PLAN OF SELECTED AREAS

BY

J. C. KAPTEYN, PH. D., SC. D.,
PROFESSOR OF ASTRONOMY AND DIRECTOR OF THE ASTRONOMICAL LABORATORY AT GRONINGEN.

PUBLISHED BY THE ASTRONOMICAL LABORATORY AT GRONINGEN.

HOITSEMA BROTHERS. — 1906. — GRONINGEN.

PREFACE.

As will be explained more fully in the introduction, the present plan was originally meant only as a working plan for the astronomical laboratory of Groningen. Having first determined on its main lines it was my task to try to convince astronomers, who were in a situation of executing the necessary plates and observations, of its urgency and thus win their cooperation.

A visit to America in 1904 and a visit to South Africa in 1905 procured a splendid occasion for this purpose. I had the good fortune of discussing the plan with a number of eminent astronomers. — In order to enable those who then showed an interest in the matter, to judge more thoroughly of the details, I worked out a provisional plan, cyclostyled copies of which were sent to them.

The result has been a good deal of additional discussion and useful suggestions by which I largely profited in drawing up the plan in its present form. A further result, I am happy to say, has been much sympathy, encouragement and promise of cooperation.

In order to enable anybody to see at a glance what further cooperation is still required, most of the astronomers who conditionally or unconditionally promised their cooperation, consented, on my request, to the publication of a few lines from their letters. These will be given below as an appendix to the plan.

To all those who helped me by their precious advice or by otherwise supporting the plan, my most cordial thanks!

First of all to those whose names appear in the Appendix. But then also, not less cordially, to Messrs Newcomb, Boss, Barnard, Schlesinger, Roberts, Backlund, Donner, Rambant, Turner, Hinks, Cookson, Easton and to the astronomers of the Cape observatory Hough, Woodgate, Power.

It is their encouragement which emboldens me now to submit the plan to, and to seek support for it in, a still larger circle.

GRONINGEN, May 1906.

PLAN OF SELECTED AREAS.

I. Introduction.

In the following pages a plan is outlined, wich may be realised by the cooperation of a few astronomers in a, relatively speaking, moderate time.

The aim of it is to bring together, as far as is possible with such an effort, all the elements which at the present time must seem most necessary for a successful attack on the sidereal problem, that is: the problem of the structure of the sidereal world.

The plan as now submitted is not considered as a definitive one. It has been worked out in so much detail in order to show its practicability. Every astronomer inclined to take some part in its realisation must be able to get a fairly just notion of the work in which he engages. For the rest however the way of its execution must be left to his own insight, in connection with the instrumental means and local conditions of his observatory.

The plan itself was conceived a long while ago. That it took its present form is due in a great measure to the following circumstance.

In Publ. No. 1 of the Groningen Laboratory I developed the idea of a general Durchmusterung for parallax. Owing to the enormous amount of work involved, the idea was not so favorably received as I hoped, but in reality, hardly expected. Sir David Gill then suggested that the best plan would be to restrict the plan to a certain number of areas distributed over the whole of the sky. This idea, only extended to other astronomical data, is embodied in the present plan.

That such a plan was evolved at the Laboratory of Groningen is only natural.

The nature of our astronomical institution makes our work dependent on that of other observatories.

Every work of a practical nature undertaken here is of necessity a work of cooperation with another astronomer who possesses the means of having photographs of the sky taken.

Thus the Cape Photographic Durchmusterung was the outcome of the labours of the Cape astronomers and ourselves.

The investigations of stellar parallax, partly contained in Publications No. 1 and 10 of our laboratory, partly not yet published, are the result of the joint labours of Prof. Donner with the observers at Groningen. So is an investigation of the proper motions of the Hyades contained in No. 14 of the same publications.

Another investigation, on the parallax of the Hyades, at the present moment under discussion here, is due to the labours of Prof. Küstner at Bonn (telescope of 5 meter focal length). Prof. Rambaut acceded to a proposal of undertaking, conjointly with myself, a new determination of certain parallaxes by means of photographs taken with the great telescope (opening 24 inch; focal length 7.5 meter) of the Radcliffe observatory.

Even the determination of parallax by means of the Meridian circle (Ann. der Sternw. in Leiden Bd. VII), though not properly a work of our laboratory, nor due to the direct cooperation of another astronomer, was only rendered possible by the courtesy of my colleague Bakhuijzen, who kindly permitted me the use of the Leiden Meridian circle.

All these investigations, except the first one, were undertaken with the express aim of settling the question: What are at present the possibilities and what the best conditions of getting parallaxes and proper motions, in a moderate time, in such numbers and with such accuracy, that they offer a fair prospect of permitting a serious attack on the sidereal problem.

At the same time, in pursuance of the same plan, I devoted all my leisure left, to the investigation of the structure of the stellar system by means of the data already available.

I thus hoped, on the one hand to obtain an insight of what data were most needed to extend our grasp on that problem and on the other hand to get definitive notions about the best means of obtaining such data.

These labours have now come to that point, that they ought to enable me to make at last definite plans for the future work of the laboratory.

After much consideration the most promising plan seems to me to be some plan analogous to that of the gauges of the Herschels.

In accordance with the progress of science, however, such a plan will not only have to include the numbers of the stars, but all the data which it will be possible to obtain in a reasonable time. My intention had been all along to give to this plan such an extension that, with the exception of the work at the telescope, the whole of it could be undertaken by our laboratory.

In working out details, however, I soon found out that, with a plan any ways on a scale meeting the requirements of the case, such would be impossible unless the funds of our institution were materially increased.

This is very serious, because the several parts of the plan are so

closely dependent on each other, that the utility of any part would be much impaired or even annulled were not the other parts achieved in accordance with it. That this is so will, I hope, abundantly appear by the perusal of these pages.

Discussion with fellow astronomers strongly confirmed me in this view. The suggestion was made, that I should work out and publish a detailed plan, in order that a greater circle of astronomers might discuss it and that, in the case that the Groningen laboratory would be unable to undertake the whole of the measurements and discussions of the photographs, some other institution or institutions might be induced to join in the work.

It is in accordance with this suggestion that these pages were written.

II. Bright stars fairly provided for.

The main consideration on which the plan rests is: that for the aim in view nothing is now so much wanted as data for very faint stars.

It is not of course maintained that what we know at present, or will know in a near future, about the bright stars, is in all respects all that can be desired.

But if we make a partial exception for the parallaxes and, in a lesser degree, for the photographic magnitudes, it will be conceded that the mass and accuracy of the data which are now available, or will be so very soon, for the brighter stars, is such, that they will certainly already allow of a pretty thorough statistical treatment.

For the **visual magnitudes**, photometrically determined, we need only refer to the monumental works at Harvard and Potsdam.

The former even contain the necessary data for finding out the systematical corrections to the visual magnitudes of the Bonn Dm [1]) and to the estimates of magnitude in the Southern Hemisphere [2]) down to magnitude 9.0.

Labours now in progress will further furnish standards of magnitude for stars down to the faintest visible in the largest telescopes.

For the **photographical magnitudes** much work has been done at Harvard College. In the Cape photographic Dm data about such magnitudes are contained for all the stars south of declination $-19°$, down to about 9.5, together with a great mass of fainter ones.

The difficulty of reducing all these magnitudes to one and the same *homogeneous* and *rational* scale has however not been altogether overcome. It is hoped that the present plan, though principally meant for very

---

[1]) Ann. of Harv. obs. vol. 24.

[2]) Ann. of Harv. obs. vol. 34.

faint stars, will also furnish data for overcoming this difficulty for stars as bright as the 8th magnitude.

For the **class of spectrum** we have the well known Draper-Catalogue, an extension of which to the South Pole is in perspective (Harv. Ann. vol. 28, part II, p. 131).

For the **astronomical proper motions** the true foundation was built up by the researches about the fundamental system by Auwers, Newcomb and Boss, which labours have been recently brought to a high degree of perfection. These labours will now enable us, for the first time, to get the full benefit of the whole of the meridian observations of the last century and a half.

Not without an enormous labour however.

But already this work is undertaken: for all the well observed stars (which again as a rule belong of course to the brighter classes of magnitude) at Albany; for the whole of the observations before 1900 at Berlin. (Geschichte der Fixsterne).

Once in the possession of these works, the investigation of the proper motion of the bright stars will enter into a new phase.

For the stars $7^m 0$ to $9^m 0$ the advance will also be very considerable; still however the accuracy of their proper motion will still be rather unsatisfactory for the purposes of refined investigation.

For the **radial velocities** the lookout is now brighter than could be anticipated even a few years ago. Already work done at or under the auspices of the Lick Observatory alone will decuple our knowledge. The possibility has been demonstrated (Lick Bullet. I, p. 25) of getting fairly accurate determinations of radial velocity "down to the eight or ninth photographic magnitude."

Several observatories are now working on the same lines or preparing to join in these researches, either for the first time or with more powerful instruments; others have only very recently finished their provisional tentative work. Considering all this, the hope seems well founded that the near future will furnish us with fairly abundant materials for the radial velocity of the bright stars.

Lastly for the **parallaxes**.

Though reliable data for the bright stars are far more scanty than could be wished, the near future is not unhopeful. Already through the labours of Gill, Elkin and others the parallaxes of the stars of the first and second magnitude have been measured with accuracy.

An extensive work by the heliometer on the parallax of the 2nd, 3d, 4th and 5th magnitude stars has been planned (see Vierteljahrsschr. Bd 36 (1902) p. 157).

To such work the meridian circles are well able to contribute largely. In a given time they can produce results certainly not inferior to those of the heliometer and they are particularly fit for work on great numbers of stars. This is proved by extensive work done at Leiden [1], at Madison (Flint) [2] and at Heidelberg (Jost) [3] and will no doubt be confirmed by other work already planned (f. i. in Königsberg).

Their efficiency will still considerably be increased by the Repsold micrometer which now begins to be extensivily used [4].

As already fairly extensive materials exist for the parallaxes of large proper motion stars (Elkin, Chase, Peter, Flint etc.) and as photography seems particularly appropriate for the fainter stars, whereas for the brighter ones it meets with serious difficulties [5], it seems probable that these labours with the meridian circle will further be directed more particularly to the bright stars.

All such work, it is true, will in general not provide us with individual parallaxes accurate within a small fraction of the whole, desirable though such a thing would be, but it will certainly furnish reliable *mean* values.

Summing up, I think that we may say, in a rough way, that the state of our knowledge of the various elements of the sidereal problem is, or will shortly be, fairly satisfactory for the stars visible to the naked eye.

For most of the elements it will still allow a thorough statistical treatment for stars as faint as magnitude 7 or 7.5.

Below this limit data become decidedly fragmentary and less accurate. They are nearly wanting altogether below the 9th magnitude.

III. Necessity of data for faint Stars.

This state of affairs must necessarily be altered, will there be any chance of getting sure foundations for our knowledge of the structure of the sidereal system.

And *first:* the importance of the faint stars for such fundamental questions as the determination of the precession is quite obvious. If we use for its determination exclusively the proper motions of those among the very faint stars for which the parallax plates bring out the smallest

---

1) Annalen der Sternw. in Leiden Bd. 7.
2) Publ. of the Washburn Obs. vol. 11.
3) Dr. E. Jost, Parall. Best. aus Durchg. Beob. Karlsruhe 1903.
4) See for instance an article by Cohn Astron. Nachr. 157 (1902) pp. 357—376.
5) These difficulties seem to have been overcome recently by a process introduced by Messrs Hinks and Russell (*vid.* Month. Not. 65, 775 and 787).

(negative) parallaxes, it seems all but certain that the difficulties, introduced by stars with large proper motion, will completely vanish.

But even if we disregard considerations of this order.

The aspect of the Milky way is mainly dependent on the distribution of the very faint stars. As a consequence every astronomer who has investigated the sidereal problem has felt this necessity of data for the fainter stars.

The gauges of the Herschels had no other purpose than to procure such data for the *numbers* of stars.

For the purpose of drawing conclusions from these, the want of photometric data is severely felt. Only quite recently an eminent investigator [1]) said about such systematic counts of faint stars of determinate brightness: „Es ist wohl „kaum eine lohnendere und wichtigere Beobachtungs-Arbeit im Gebiete der „Stellar-Astronomie anzugeben."

I do not think therefore that anybody will dispute the necessity of getting reliable data for the numbers and magnitudes of stars as faint as can be obtained by a moderate labour.

That this general conviction extends to other data is most convincingly proved by such an undertaking as that of the *Carte du Ciel*.

The immense labour involved in the production of a catalogue of all the stars down to the 11th magnitude cannot have another significance. The mere numbers and magnitudes of the stars down to the 11th magnitude or even down to the 14th magnitude, as in the longer exposure series, would have been obtained more satisfactorily by a small fraction of the work now expended.

The main aim of the catalogue must therefore be to furnish, at some future time, the basis for the investigation of the proper motions of the fainter stars.

That the urgency of such further elements, of which the proper motion is but one, has not been more generally discussed up to the present time, must be attributed to the seemingly despairing extent of the work involved.

For just this great undertaking of the *Carte du Ciel* seems to show, clearer than any thing else, that the determination of all the elements, for all the stars contained in the maps of the *Carte du Ciel*, for which such a determination is well within the reach of our present methods, is a task too big even for the combined efforts of all the astronomers of the world.

For, how long will we have to wait before the whole of the catalogue

---

[1]) Seeliger, Betrachtungen über die räuml. Verth. der Fixsterne. Abh. der K. Bayer. Ak. der Wiss. II cl. 19 Bd. 3e Abth.

of stars down to the 11th magnitude will be in the astronomers hands? And even if it is, when will it become of any extensive use for the proper motions?

Not before, at the end of some 50 or 100 years, the work will be repeated and compared with the present catalogue.

Even for the magnitudes and numbers of stars it will be of little use for the sidereal problem, before, by new methods and new labours, means have been created to reduce the whole of the magnitudes to a rational and homogeneous scale.

I may say at once that it is just one of the aims of the present plan to furnish such means.

The completion of that part of the *Carte du Ciel* which must furnish us, not with catalogue places but with maps of the sky, down to about the 14th magnitude, seems still much further off. — Even if finished it will yield no proper motions. For the magnitudes it will labour under the same difficulty as the catalogue part, immensily increased by the fact that the magnitudes are not expressed in numbers but must be taken from the maps. — For this part too the overcoming of the main difficulty will require the execution of just such a work as that proposed for the photographic magnitudes in the present plan.

If in addition to data about photographic magnitudes, numbers and proper motions, we consider that data about parallax are certainly as urgently needed; that the spectra, visual magnitudes, radial velocities have as great a claim to our attention as the other elements, the prospect of such an undertaking aiming at completeness, seems fairly despairing [1]).

Happily, for the main purpose, for the statistical treatment of the problem, we may certainly restrict ourselves to less exhaustive labours.

Even if we possessed complete data, it would doubtlessly be found that, in order to make it possible to get a good insight in the general traits of the stellar system, it would be as good as unavoidable to condense our materials into a certain number of what might be called *normal* regions.

If this be true, then 99 percent of the whole labour may be saved by restricting ourselves to such normal regions from the outset.

Detail will certainly be lost. The aim of the present investigation

---

[1]) To prevent any possibility of being misunderstood in my appreciation of the *Carte du Ciel* work, I desire to express my perfect concurrence with the opinion of those who see in this work a *duty* to posterity. A hundred years hence this *complete* and *accurate* survey of the sky will be a treasure to astronomers, which nothing could replace. Great part of its value will be due to the fact that it was undertaken almost at the earliest possible epoch.

cannot therefore be a very detailed study of the Heavens, but rather a study of their most prominent and general features [1]).

The gauges of the Herschels have been found extremely useful in studies about stellar distribution. That they do not more fully satisfy our needs, is *not* a consequence of the fact that they only cover 145 square degrees of the 41000 contained in the firmament, but that they are restricted to this *one* element: *total number of stars.*

Did we possess these numbers for *each separate, sharply defined magnitude,* then already an immence step forward would be possible, as is well shown in Seeligers investigation: „*Betrachtungen über die räuml. Vertheilung der* „*Fixsterne*".

Still our need of the other elements is certainly not less urgent.

IV. Scope of the Plan.

Before entering on the discussion of these elements, however, it seems expedient to give first an outline of the actual plan.

It can be best described as a series of gauges, arranged in a manner somewhat different from that of the Herschels, in order to meet the exigencies of photography and extended to other data which have come within the reach of the practical astronomer since Herschel's time.

It consists simply in this: *For* 206 *areas, regularly distributed over the sky and for another, little extensive series of particularly interesting regions, to obtain astronomical data of every kind, for stars down to such faintness as it will be possible to get in a reasonable time.*

A more precise statement follows for the several elements: Let L be the limiting magnitude which it will be found practicable to photograph in a reasonable time.

Then our plan in its fullest extent would embrace, fitly distributed over the whole of the areas:

A. For some 200000 stars [2]) the *rough positions* and sharply defined *photographic magnitudes.*

---

[1]) That there is something more or less regular underlying the structure seems certain. In this connection I may quote Newcombs conclusion (Stars p. 276):
„If we should remove from the sky all the local aggregations of stars, and also the „entire collection which forms the cloud-forms of the Milky Way, we should have left a „scattered collection, constantly increasing in density toward the galactic belt."

[2]) According to Seeligers „Betrachtungen" p. 592 the total number of stars in the gauges of the Herschels is 117600.

All the stars, down to magnitude L, within areas of accurately known extent must be included, in order that the complete number of stars per square degree may be found for every class of magnitude.

B. For the same 200 000 stars: *visual magnitudes.*

C. For some 20 000 out of these 200 000 stars, fitly distributed over the magnitudes: bright to L: accurate *proper motions.* Individual errors not to exceed 0″.01 in the p.m. in each coordinate.

The following distribution over the magnitudes may serve as a basis for further discussion:

Magn.	Number.
7.0— 8.0	200
8.0— 9.0	600
9.0—10.0	2000
10.0—11.0	4300
11.0—12.0	4300
12.0—13.0	4300
13.0—14.0	4300
	20000

D. For the same 20 000 stars: *parallaxes* with individual p. e. not exceeding 0″.02.

E. For as many of these 20 000 stars as can be obtained and for as many more as will be found possible, in a wider area of the plate: *class of Spectrum.*

F. For as many of these stars as possible: *radial velocity.*

G. *Total amount of light received from different parts of the sky.*

The necessary extent of the areas may be evaluated as follows:

In the gauges of the Herschels the mean number of stars per square degree is 813.

From 39 plates of the Paris observatory (3 exposures of 30 min. each) I find this number to be about 450. These numbers were obtained by deriving, first the density for various zones of galactic latitude and then combining these with weights proportional to the extent of the zones.

It seems therefore, that the limit L may be fairly placed at such a value that the mean number of stars pro square degree will become 500.

With a telescope of somewhat greater aperture than that of the Paris observatory (12 inch) this limit will only require moderate times of exposure; and even with instruments of the *Carte du Ciel* type an exposure of slightly over 30 minutes will be sufficient.

In order to get a total number of 200 000 stars, the whole of the 252 areas (about the total number of areas see further below) must therefore

cover 400 square degrees; which gives about 1.59 square degree for the extent of every area.

I will admit in what follows, areas of

$$75' \times 75'.$$

[Eventually it may be useful to substitute a circle with radius 42'].

Such a field is covered by the best part of the plates produced by most of the present photographic telescopes.

It will be understood that the divisions E, F, G, which will not embrace stars of such faintness, cannot well be limited to such small areas.

V. Urgency of the work.

I will now proceed to say a few words on the urgency of such a work.

The total area covered will be 400 square degrees with 200 000 stars, as against 144.6 square degrees with 117 600 stars in the gauges of the Herschels.

It is true that the gauges are distributed over a greater number of points of the sky.

This is certainly a disadvantage of our plan.

It is a consequence of the necessity of confining the number of photographic plates to be taken to a reasonable number.

As a consequence of this restriction to 252 areas a further increase in the number of stars beyond 800 pro area, would seem to promise no real advantage.

If we can obtain very large plates with uniform images over an area of say $6° \times 6°$, then the disadvantage can already be made to disappear, because we can then distribute the same number of measurements over some 5 or more parts of the plate. — With smaller plates or with insufficient uniformity of the images such an arrangement is impossible or undesirable. In this case further improvement must be left to a future extension of the work.

Our plan will accomodate itself quite naturally to such an extension by intercalation of an equal or double number of plates.

Already just now we considered the importance of knowing the numbers of the stars of various magnitudes. Further on we will discuss the difficulties of getting a homogeneous and rational scale. If they can be got over, the *photographic* magnitudes have the advantage over the *visual* ones in the rapidity and accuracy with which they can be obtained.

Moreover the division A of the plan, once executed, will furnish the means of reducing the magnitudes of the Cape Photographic Durchmuste-

rung to a rational and homogeneous scale. It will render the same service for the magnitudes of the *Carte du Ciel.*

If therefore one of the divisions *A* or *B* must be sacrificed, there can be little doubt that we will best give up the latter.

In the meanwhile the advantages of having *both* of them accomplished are obvious.

The difference:

Photogr. visual magnitude

is a natural and very convenient measure of the colour of the stars, an element that it is very difficult to obtain in another way.

It enables us to answer such questions as: Is it true that the stars in the Milky way are bluer than those at considerable galactic latitudes? (About this question see: Bull. de la Carte du Ciel, vol. II, p.p. 131—158).

That several parts of the Milky way are systematically different in colour as maintained by de Sitter? (see Publ. of the Astr. Lab. at Groningen No. 12.)

The bearing of such questions on the problem of the structure of the universe is obvious.

Moreover, for stars too faint to allow of a direct determination of their spectra, this difference of the two magnitudes is the only clue to its nature. It will naturally take the place of the determination of spectrum for these stars; so for instance in an investigation of the relation between spectrum and proper motion, which for the brighter stars is so striking (see Monck Astrophys. Journ., vol. II, p.p. 253, 389, 700, 874; further: Versl. Kon. Ak. Amsterdam, Apr. 1892; Jan. 1893).

For the rest, the importance of a homogeneous scale of the visual magnitudes of faint stars, distributed at convenient intervals over the whole of the sky, is already so well recognised that steps have actually been taken to provide such a scale for stars down to the faintest visible in the largest telescopes. Ours would be only an extension of that plan.

The importance of the *proper motions* for the sidereal problem need not be demonstrated.

If from the apparent distribution of the stars we wish to pass to the real distribution in space, we must know the distances.

Again we need these distances if from the apparent magnitude we wish to pass to the real illuminating power (*luminosity*).

As, with a few exceptions, the distances of the stars are unknown and excessively large, we are driven to derive these distances in the main from the parallactic motions.

The use of the parallactic motion as a gauge of distance was recognised

long ago. Its paramount importance was most strongly advocated by Prof. Newcomb (Astr. Journ.. vol. 17, p. 41) and by myself.

As soon as we will have sufficient data for the radial velocities there will be another way: the comparison of the radial velocities expressed in kilom. pro second with the astronomical proper motions expressed in seconds of arc.

Both methods suppose the knowledge of the astronomical proper motions.

True: just in the present plan an attempt will be made to get the materials for getting *direct* measurements of the mean distances. But *first:* these determinations can only furnish relative parallaxes, so that, in order to get the absolute parallaxes we must still draw on the parallactic motion. *Second:* on account of the excessive smallness of the quantities sought and the consequent extreme difficulties of their determination, we certainly ought to neglect no way which may help us to get at good results.

Moreover the proper motions are absolutely indispensable for the determination of the direction and velocity of the real motions of the stars in space.

The urgency of extending our knowledge of accurate motions to the *fainter* stars is as little doubtful.

Already the fact that the bright stars with appreciable proper motions show no connection with the Milky way, whereas for the same stars having motions of the order a few seconds pro century this connection is strongly marked (Versl. Kon. Ak. v. Wet. te Amst. 1893, p. 130) seems to call for a similar investigation for the fainter stars.

More generally, however, the same or analogous reasons which make it necessary to include the fainter magnitudes in investigations based on the number of stars of various magnitudes, make it necessary to include them in investigations about proper motions.

Whosoever makes a study of the stellar distribution will at every step have to contend with the want of data for the fainter magnitudes. I may not enter into details here, however, and must refer for these to a paper, shortly to be published, read before the congress of arts and sciences of St. Louis.

Also a perusal of Publ. No. 11 of the laboratory at Groningen and a short paper by myself in Astron. Journ. No. 566 will show how our knowledge about what there is called the *luminosity curve*, the *stardensity* at different distances from the sun, the amount of *absorption of light* in space, all fundamental elements in the discussion of the sidereal problem, will depend, either wholly or in great part, on our knowledge of the proper motions of very faint stars.

The parallaxes. The want of directly measured paralaxes for the study of the sidereal problem is probably more generally felt than that of any

other element and their need for the faint stars as well as for the bright stars need not be demonstrated. It will be only necessary to examine what can be attained by our measures.

This will be done further on.

Meanwhile it may be well to call attention to the fact, that the execution of a plan like the present one offers many secondary advantages. One of the most important of these will be, that it will decisively show what may be expected of a general Durchmusterung for parallax and what will be the best conditions for the execution of such a work.

Class of Spectrum. Several recent investigations have brought out the fact that the apparent distribution of the stars of different spectral type is extremely different [see Ann. of Harv. Obs., vol. 56, no. 1; Verslagen Kon. Ak. van Wet., Amsterdam 1893, p. 125—140].

There is a very striking difference in the arrangement in regard to the Milky way.

Not only this, but the proportions between the numbers of the 1th and 2nd type stars (Secchi's notation) varies regularly with the amount of proper motion, between limits ranging from 0.6 to 19 *at least* (l.c. p. 133).

It thus becomes indispensable to investigate the distribution in space of the stars of different type separately.

How inadequate the data at present are for such investigations even of the bright stars of type I, appears from attempts already made in this direction; *vide*: Publ. of the Lab. at Groningen, No. 11, p. 24—32.

How different things would be if we had reliable data for the class of spectrum of all the stars in our areas, down to, say, magnitude 9.5, for all of which, or nearly so, the magnitude, proper motion and parallax would also be known!

Radial velocities. There seems little doubt but that the radial velocities, which are determined in kilometres pro second, independently of the distances, must soon become the most promising element in studies about stellar distribution.

The full benefit of it will of course be reaped only when these determinations will have been extended:

1st to great numbers of stars;

2nd to much fainter stars than included at present.

The discovery by Campbell, that the brighter stars have exceptionally small radial velocities [1], is in itself sufficient to show the necessity of the 2nd condition. The first needs no comment.

---

[1]) See Astrophys. Journ. vol. 13, p. 85. The same fact is brought out by the astronomical p. m. (*vid.* Astr. Nach. No. 3487, p. 104). It is there seen that the motion does *not* gradually change with the magnitude, but that the stars 0—3.5 are exceptional.

It is to be feared that, within the limits of our areas, a sufficient number of bright stars may not be found.

In this case it still would seem that the stars of our areas would deserve the first attention, because the importance of the results will of course be greatest for those stars of which the other elements: proper motion, parallax . . . . . have also been determined.

The enormous importance of these radial velocities has enboldened me, in what follows, to call attention once more to the methods proposed to obtain them by the wholesale.

Total amount of light received from different parts of the sky. I have added the determination of this element, because it seems one of very great importance for our problem. Recently Newcomb drew attention to this element and even gave some first results. (Astrophys Journ., vol. 14, pp. 297—312). It ought to teach us something about the stars too faint to be included individually in the plan. It will be difficult, as well as undesirable, to restrict the observations to the areas of our plan.

At various times I have tried some experiments and have lately designed a little instrument which seems to promise well.

After having thus, very imperfectly, explained the urgency of such a plan as the present, I will now discuss its feasibility and what, after a good deal of discussion with other astronomers, seem to me the best conditions for its execution, inviting astronomers to make suggestions as to possible improvements.

VI. Choice of the Areas. Systematic plan.

About the best choice of the areas opinion appears to be somewhat divided.

Whereas all the astronomers consulted agree in the demand for a uniform distribution of a good part of the areas, some would absolutely restrict them to such uniformly arranged positions, on the ground that only such a plan might be expected to lead to the knowledge of the *general* laws governing the structure of the sidereal system and that these must be found out before divergences from the rule are to be studied.

Others urged that in this way just the most interesting parts of the sky, which would seem "to have had an important influence on the structure of the Milky way" [1]) would be excluded.

Prof. Pickering proposed to have two sets of regions:

1st "nearly uniformly distributed for statistical purposes" (*systematic plan*),

---

[1]) From a letter of Prof. Edw. C. Pickering.

2nd „special regions, generally maxima and minima, rather than „average regions (*special plan*); ..... the number of these special regions need "not be large", finally commanded the assent of all the astronomers consulted, in particular of those who, up to the present time, promised to take an active part in the work.

A number of not over 30 or 35 special regions was considered fair. — For reasons which will presently appear I have somewhat enlarged the number. On the other hand parallax and proper motion plates are only proposed for part of them.

About the number of plates in the *systematic* plan opinion was again divided. — In trying to satisfy all the conditions (especially that of having the centres at declinations which are a multiple of 5°) we get either a total number of about 200, or a number of between 100 and 120 areas, whereas a satisfactory plan with an intermediate number seems not so well feasible. Now some were of opinion that, for some parts of the work at least, the former number might be found too large on account of the work involved. On the other hand the latter number was pretty generally deemed insufficient for a fair representation of the whole of the sky [1]).

A proposition of Sir David Gill reconciled the two views.

With a slight modification, which proved necessary in working out the idea, it comes to this. The 206 areas shall be divided into two classes. In case of any doubt as to whether a certain part of the work can be achieved completely, the first class areas, which must form a complete set by themselves, will be first undertaken, leaving the question open, whether the second class areas will be undertaken later on, or not.

In the list of centres annexed to this paper, the centres of the first class areas, which will take precedence, have been distinguished by asterisks.

Total number of 1st class areas . .	118
„ „ „ 2nd „ „ . .	88
Total	206

The main conditions to be satisfied in the selection of the *systematic* centres were considered to be the following:

1st Uniform distribution over the sky;

2nd Arrangement along the parallels of declination

0°	+ 15°	+ 30°	+ 45°	+ 60°	+ 75°	+ 90°
	— 15	— 30	— 45	— 60	— 75	— 90.

1) W. Herschel counted 1088 fields. J. Herschel no less than 2289. The two sets have a very considerable overlap. Between 0 and 90 degrees of Southern galactic latitude the fields counted by J. Herschel alone number 1694.

This arrangement presents the two advantages:

a. standards for visual magnitude (down to 9.0) are provided for these zones. For those of declination — 15 to + 90 by Pickering's photometric revision of the Bonn Dm. (Harv. Ann. 24); for those of declination — 30 to — 90 by Bailey's catalogue of 7922 Southern stars (Harv. Ann. 34).

b. Some of these declinations (+ 15, + 30, + 75) lie at the overlapping positions of the Zones observed at different observatories for the Astron. Gesellsch. Catalogue. As a consequence thereof, there will be quite a great number of stars on the plates of which the positions for 1875 are known with exceptional precision.

The advantage of this fact for the derivation of standard proper motions is obvious.

3d As far as possible a *regular distribution* of the plates *in regard to the Milky way.* — As this belt is cosmically fundamental and moreover its structure more diverse than that of other parts of the sky, it seems desirable to have two plates of every zone (from $\delta - 60$ to $\delta = + 60$) fall as nearly as possible just on the middle of the Milky way.

4th An arrangement such that the *right ascensions* of the centres of the totality of the plates, both for the Northern and for the Southern Hemispheres, be somewhat *evenly distributed* over the 24 hours. Observations of the standard stars in the meridian and also the taking of the plates near it, will thereby be enormously facilitated. I have endeavoured to obtain this even distribution approximately for both hemispheres *including* in both cases the equatorial belt of areas.

5th Centre of each area, as far as possible, coincident with a star of maguitude 8.0—9.0. Several astronomers urged the desirability of having a bright star, which will serve as a guiding star for most of the observers, at the centre. Prof. Pickering pointed out, however, that in the photographs, as well as in the visual observations, the magnitude of a faint object is influenced by the vicinity of a bright star. This objection prevents the use of stars brighter than 8.0. Those between 8 and 9 cannot be avoided in any case. In any case there will be, on an average, somewhat over 3 of these stars on every area of 1.5 square degrees. Moreover the present plan ought certainly to provide ample data for stars of this magnitude. There thus can hardly be any objection against such a star at the centre. On the other hand stars fainter than 9.0 are undesirable as guiding stars for all but the most powerful telescopes.

The list of centres given at the end of this paper, will be found to comply fairly well with the conditions.

It needs no lengthy explanation.

In order to be sure about the magnitudes I have given three good authorities in most cases; in the first place the photometric magnitudes according to Harv. Ann. Vol. 24 and 34 (H. 24 and H. 34); further the Bonn Dm. (BD) and the Astron. Gesellsch. Catalog (AGC). For the southern Hemisphere: Thomes Dm. (Th.) and the Cape Photographic Dm. (CPD). — In many of the cases, where one of these authorities is wanting, I have given, in the column for that authority, a reference to other sources distinguishable by the following letters:

A Argelanders Southern Zones,
K Königsberg Zones (Weisse),
B Bonn Meridian obs. (Bonn VI),
Sj Schjellerup,
G Cordoba General Catalogue,
Z „ Zone „

In comparing these magnitudes it is necessary to remember that

9.2 Harvard = 9.0 BD.

VII. Special plan.

The choice of the areas for the special plan proved extremely difficult.

Everybody agreed that no areas ought to be included which do not promise some contribution to the question of the *general* structure of the stellar system.

Of course the study of the individual clusters is highly interesting, but it falls outside the limits of the present plan, which cannot be indefinitely extended.

Still, as soon as we include somewhat exceptional regions, it is very difficult, if not impossible, to say which of them promises to contain a clue to the plan of the general structure, which do not.

I have availed myself of the very valuable advice of several eminent astronomers (Pickering, Hinks, Easton and others). In particular Hink's advice to include regions where the Milky way shows a sharp edge has been largely followed. Still a somewhat satisfactory choice would certainly have been impossible without the help of Prof. Barnard, who, with extreme courtesy, presented me with a series of admirable photographs, obtained with the Bruce telescope of the Yerkes observatory, covering the greater part of the M. W. It is deeply to be regretted that we do not as yet possess a similar series for that part of the southern sky, not accessible to the Yerkes astronomer. But even provided with these maps a rational selection is not possible, as long as we have no definite idea about the

2

nature of the data we may reasonably hope to be able to obtain from the photographs.

Of course we may photograph the very rich and the very poor parts of the Milky way and we may hope to learn something by counting the numbers; but will there be any special advantage gained by photographing and measuring the parallaxes and the proper motions of these regions? The question is important, because just these parts of the programme will be the most laborious.

The question is somewhat different here from what it is in the case of the systematic plan, because the number of plates devoted to any particularity is necessarily restricted.

The following considerations are submitted as the best I could think of. The special plates must of course in the main be devoted to the study of the Milky way proper. — Now the most striking feature of the Milky way is no doubt its cloudlike appearance. Very rich parts are separated by much darker gaps, in which the stars are far rarer, in some cases nearly wanting.

We further meet with fissures and black holes, in the midst of very rich parts of the Milky way, which seem extremely puzzling.

It can hardly be doubted but that we would be in a far better condition for the understanding of the real significance of this structure, if we knew something about the distances.

Now, not in order to discuss any particular theory, but merely to illustrate the relative importance of certain data by a particular case, let us imagine that we want to inquire whether the dark gaps, in the midst of rich parts of the Milky Way, are caused by clouds preventing the light of the more remote stars to reach our eye.

If the theory is correct, we have to expect that the rare stars, which we still see in these gaps, are really much nearer to us, that they are in fact on this side of the cloud. — On the other hand, if we find the stars in the gaps on an average as distant as those of the rich adjacent parts, then the theory would seem to be untenable.

The question therefore presents itself: may we reasonably hope to get some definite notion about these distances by the plates of the special plan?

It can be clearly demonstrated [1]) that, whereas the stars of mean magnitude 5.5, with proper motion exceeding 0″055, are distributed fairly uniformly over the whole of the sky, the stars of the 1st Type (by Secchi's

[1]) Verslagen en mededeelingen van de Kon. Akad. te Amsterdam Jan. 1893. An abstract in English of this paper has appeared in *Knowledge* 1893.

notation) with smaller p. m. and also the stars of the 2nd Type [1]) with p. m. below 0″035, are far more numerous in the lower galactic latitudes.

There thus cannot be any reasonable doubt but that the stars of magnitude 5.5, having p. m. of 0″035, are already well in the Milky way proper.

Now we may fairly estimate the average parallax of such stars to be about 0″009 [2]).

It is more difficult to estimate the average parallax of *all* the stars we see in rich parts of the Milky way down to, say, the 14th magnitude. Meanwhile it seems exceedingly improbable that it can exceed 0″003. In order not to present things in too favorable a light, we will suppose this parallax to be 0″004 (which indeed is about the parallax of the stars 10.5 [3])).

If, therefore, we imagine that in a dark gap, a dark cloud covers the Milky way proper, there must be a difference of average parallax of the stars visible in the gap (and supposed to be on this side of the cloud) and of those surrounding the gap, of at least

(*a*) . . . . 0″005.

With Campbell's velocity of the solar system through space [4]) (4.20 solar distances yearly) the relative parallactic motion corresponding to this relative parallax is:

(*b*) . . . . 0″021.

It cannot be denied, of course, that systematic motions of the stars may mask to a certain extent the effect of the parallactic motion, and may therefore prevent us from drawing legitimate conclusions about the distances from components of the p. m. towards the Antapex. But

1st. The *systematic* plan must furnish us with data about the relation between this motion and the parallaxes for different parts of the sky;

2nd. The *special* plan, as here proposed, will no doubt furnish sufficient materials for the determination of the position of the Apex exclusively from the areas now under consideration. If it turns out (as there is reason to expect) that this Apex coincides nearly with that of the whole sky, then the probability that the average motion towards the Antapex will be a measure of the parallax will be very strong indeed.

The question thus ultimately becomes: what is the prospect of measuring

---

1) The stars of other types are relatively speaking so rare that they need not here be considered.

2) Publ. of the Astr. Lab. at Groningen N°. 8.

3) See for instance Astrom. Journ. N°. 566, p. 118.

4) Astrophys. Journ. Vol. 13, p. 80.

the quantities (*a*) and (*b*) with accuracy by a very moderate number of plates.

Suppose, taken on the same plate, a dark place of the Milky way together with the surrounding rich parts. We may thus have the best prospect of avoiding systematic error.

Suppose that we are able to measure the parallax of *one* star in a dark gap, relative to *one* star in the rich parts, with a probable error of [1])

$$\pm 0''04.$$

In order to measure the quantity (*a*) with a probable error of, say, *a fifth* of its amount, we will have to measure 1600 pairs *i. e.* 3200 stars in all. With 90 stars measured on each plate and assuming that half this number can be seen and measured in the gaps, we will still need 35 plates for the purpose.

I conclude that, though the matter be not absolutely hopeless, the lookout is not very bright.

The *relative* p. m. of two stars on the same plate, being independent of the standard p. m., may be determined with high precision.

With a difference of epoch of 10 years, this p. e. may be assumed to be

$$0''0075 \sqrt{2} = 0''0106$$ [2]).

In order to sin on the safe side I will take

(*c*) . . . . $\pm 0''013$.

This value, yielded by two stars, is already considerably smaller than the quantity (*b*) sought. In order to determine this quantity with a probable error *five* times smaller than its amount, we will want 10 pairs of stars, *i. e.* only a total of 20 stars in all.

The errors of observation will therefore not be of the slightest consequence.

They would not prevent us from determining the quantity (*b*) with all desirable precision, even if it were several times smaller than it is.

Meanwhile we have to consider that, in order to get the true parallactic motion, we have to eliminate, not only the errors of observation, but also the *peculiar* p. m. themselves and it may reasonably be urged that, in the supposition here made, *viz* that the stars seen projected on the gap are relatively near to the solar system, we may even expect relatively large p. m. Now let us suppose the mean value of a component of the pecu-

[1]) About this estimate see further below. I have here taken the value somewhat greater on account of the less perfect mixture of the stars to be compared.

[2]) Publ. of the Astr Lab. at Groningen N⁰. 14 p. IX. The estimate is valid for instruments of the *Carte du ciel* type and exposures *not* in the same hour-angle.

liar motion to be 0″06 [1]). The prob. value will be something about ± 0″05.

Now, even with such p. m. playing the part of errors of observation, in order to get a prob. error five times smaller than (b), we will only require a total number of 140 stars.

If the mean p. m. of the stars, seen projected on the gap, prove to be still larger then:

1st: their mean parallax will undoubtedly be much larger than here supposed, so that, instead of becoming more difficult, the determination of the ralative p. m. must become easier;

2nd. great part of the p. m. must be so great that their *individual* amount will be determined with some precision. In this case the total proper motions themselves will furnish a good criterium of distance.

We thus come to the conclusion that, for the purpose in view, there is little to encourage us to take parallax plates. — The p. m. plates on the other hand promise to be of the greatest help in solving questions about the cloudlike structures of the Milky way.

We may take a more general point of view. The parallactic motion must remain measurable with precision for distances several times larger than those corresponding with the quantities (a) and (b). This will already be sufficient to judge of such theories as those that assume that the Milky way is a stratum relatively very thin in the visual ray, or a spiral with a somewhat circular section or the theory which sees in it a moderate number of cloudlike structures, the dimensions of which in the visual ray are quite of the order of the transverse dimensions. In addition to many other special points of interest, the dark gaps in the Milky way offer this advantage for the accurate study of the p. m., that we may probably choose the stars *in* the gaps and in the surrounding parts of equal brightness. This will abolish the only possible source of systematic error.

Meanwhile, in the selection of the areas, we have to consider that the parallactic motion is proportional to the sine of the distance (λ) from the Apex. The considerations made just now properly apply to values of λ near 90°. The conclusion will not change, however, as long as λ exceeds 30°. Regions at smaller distances from the Apex or Antapex will be of little use for our purpose.

It might be urged that areas very near the Apex and Antapex would be also very interesting, because they will show the p. m. *un*modified by the motion of the solar system. — I have thought however, that, for this purpose, the 8 plates of the *systematic* plan, within 15 or 16 degrees from the Apex or Antapex, would be sufficient.

---

[1]) This is the mean value of the component τ for the whole of the Bradley stars (mean mag. 5.5), as may be derived from table 2 Astr. Nachr. N⁰. 3487 p. 105.

For the reasons given I have introduced in the special plan:

A. Quite a number of areas (19) in which Barnards or Wolfs [1]) maps show a black opening surrounded by rich parts (Nos. 2, 5, 9, 12, 24, 25, 37, 38, 40, 41, 44 of the list); or a rich part, or rich parts, between dark spaces (Nos. 29 [2]) (η Argus), 36, 39, 43, 46); or where there is at least a sudden change in star density (Nos. 17, 30, 31).

Only one of these (No. 5) is within 30° from the Apex or Antapex ($\lambda = 28°$).

In addition to these areas of strong contrast in stardensity, there have been included in the plan:

B. A series of areas on the great branches of the Milky way and the rift between them. For this study have been introduced: just before the division, No. *33*; then: on 1st branch: Nos. 42, 45, 1, 2 (the last also in *A*); on the 2nd branch Nos. 35, 3, 7, 8; on the rift Nos. 34, 4, 6. These last may be strengthened by the plates of the systematic plan: *18h 9m — 15°; 18h 36m, 0°; 19h 10m + 15°*. Moreover several plates of the series *A* will very usefully serve the study of this division of the Milky way.

C. Further: very rich or very poor parts of the Galaxy:

Rich: Nos. 10, 18, 23, 28.

Poor: Nos. 11, 15, 16, 19.

Here too many of the plates of series *A* will be useful, so for instance No. 31 on the border of the southern Coalsack.

D. Two Milky way areas (Nos. 26 and 27) for which DE SITTER finds a marked difference, in opposite direction, between photographic and visual magnitude.

E. For the rest I have included, of *extra*-galactic regions:

two areas (Nos. 13, 14) coinciding with the richest parts of the Nubecula minor;

two areas (Nos. 21, 22) coinciding with the parts richest in stars and nebulae of the Nubecula major; [3])

one area (No. 20) coinciding with the part of the Orion nebula with strongest contrast in stardensity;

---

1) Next to the maps of Barnard, some photographs of Wolf and the isophotic map of Easton (Verhand. K. Ak. v. Wet. te Amsterdam, 1st sect. Vol. 8 No. 3) have been of the greatest help to me.

2) This area might as well be classed with the preceding ones.

3) In the *systematic* plan there is no plate covering part of either of the Nubeculae. The plate $0^h 20^m - 75°$, which comes very near to the Nubecula minor, still keeps just free of it.

one plate (No. 32) covering a part of the sky, near the North Pole of the Milky way, exceptionally rich in small nebulae.

For reasons given above it seems desirable to take p. m. plates for the areas of series *A* (19 plates) but *no* parallax plates. For the *two* plates *D* also it would be useful to have p. m. plates, for this reason: The difference of colour, if real, must mean difference in spectrum and corresponding therewith we must expect difference in the amount of p. m.

For the regions *B* and *C* (19 plates) parallax plates are proposed, mainly for the purpose of strengthening the parallax determination for the Milky way in the *systematic* plan, which in this belt contains only 20 plates. For the purpose of the systematic plan, *i. e.* for obtaining good averages, the plates of series *A* are less fit, because they are chosen in places of very local and exceptional character.

The *six* regions *E*, finally, are relative to extra-galactic interesting parts of the sky. The Nubeculae; the nebula of Orion; a remarkable nest of small nebulae near the Pole of the Milky way. For all these regions p. m. plates seem highly desirable. If we succeed in getting very numerous nebulae on these plates (Wolfs photographs contain about 400 nebulae on an area of 75′ × 75′ around our centre) it is not at all improbable that indubitable p. m. and, with them, notions about the distances of these nebulae may be obtained. For Wolfs region even a couple of parallax plates might not be out of place.

For all the areas of the special plan, Durchmusterung plates and, if feasible, plates for the classification of the spectra seem recommendable.

In conclusion it need not be said that, with the areas included in the list, all the remarkable areas of the sky are by no means exhausted. But in many cases the *systematic* plan will furnish the materials for the study of special objects not sufficiently provided for in the special plan. So, for instance, this plan will furnish *eight* areas specially rich in nebulae in the constellation of Virgo and some others in Pegasus and Triangulum. So again that plan will furnish sufficient data for the study of Helium stars etc.

Meanwhile some areas of more than usual interest must no doubt have been overlooked.

Any suggestion for change or addition, made in time, will be received with the greatest gratitude.

The list of special areas appended at the end of the present paper needs little explanation. The areas in the Milky way have been arranged in the order of the galactic longitudes, which coordinates have been inserted in the 10th column.

The columns 2—7 contain the magnitudes according to several autho-

rities. The same letters have been used in the abbreviations as in the systematic plan. The 11th column contains the galactic latitude.

For completeness sake the galactic coordinates of the Milky way areas of the *systematic* plan have also been inserted in the list. The 12th column contains an estimate of the intensity of the galactic light. For the Northern Hemisphere these estimates were taken from Eastons *Carte isophotique* [verhand. Kon. Ak. van Wet. te Amsterdam, 1st Sect. vol 8, No. 3].

I represents a very faint, though still perceptible, Milky way light;

VI indicates the most brilliant light;

Nos. II, III, IV, V represent intermediate shades.

For the parts not covered by Eastons maps, and not described in the remarks, I have given only general indications, in conformity with the Atlas of Goulds Uranometria Argentina.

Column 13 contains a reference to the maps on which the choice of the area was made. The maps referred to under the names of Barnard and Wolf are photographs; those of Herschel and Easton are drawings.

The quantity λ contained in Col. 14 denotes the distance of the centre from the Apex (adopted position for 1875 $\alpha = 273$ $\delta + 29°5$).

For a description of the particulars of the several areas the remarks at the end of the list must be consulted.

VIII. Durchmusterung plates.

For getting the total number of stars for each magnitude pro square degree and further for enabling us to make a good choice of the objects for p. m., parallax etc., the first thing to obtain must be good Durchmusterung plates.

If the limiting magnitude is about the 14th, then, as already suggested above, we might take a field of $75' \times 75'$ for thorough Durchmusterung. The position of every individual star on this area should be roughly determined, the diameter accurately estimated.

Meanwhile it would of course be very highly desirable: *1st* if for this *Durchmusterung* the limit of magnitude could still be lowered, even though it be pretty well impossible to lower this limit for the other parts of the work; *2nd* if a really good field of greater dimensions were available.

Of course, if we include fainter stars, practical reasons may compel us to lessen somewhat the dimensions of the areas, which must be completely surveyed, at least in the rich parts of the sky. — As long, however, as the extent of the field is exactly defined there can be no serious objection.

But, even when the areas completely surveyed are small, there is this great advantage in plates covering extensive areas, that we are always

able to extend our countings, wherever preliminary results drawn from our measures, seem to need strengthening or confirmation.

In future studies, especially of the Milky way regions, this possibility of extending our investigations to extensive areas, for which the scale of magnitude will be accurately known, must be invaluable.

It must be considered to be matter of extreme gratification therefore, that Prof. Pickering has offered to devote the Harvard Bruce-telescope to this work. There probably is not another instrument in the world, in every way so particularly fitted for the purpose.

IX. Visual magnitudes.

The question to be answered for the various portions of the sky is: what is the number of stars of any photometric magnitude per square degree.

Already in what precedes I remarked that it will be by no means an easy task to obtain visual estimates for the whole of these stars, especially as they will require a telescope of probably not below 15/inches aperture.

A good plan for the observation would seem to be to arrange the work somewhat in the way of that for the Bonn or Cordoba Dm. Only the Zones swept over would of course have to be taken much narrower.

Not so much for the purpose of proposing some definite plan, which is certainly better left to the astronomer who will make the observations, but merely to illustrate its feasibility, let us assume that for an area of an extent of 75′ in declination and 5^{min} in R. A. (in the Equator), we let the stars of a Zone of 4′ in breadth transit over a fixed, faintly illuminated wire. The time of transit will furnish the right ascensions; the declination would be obtained by estimating the height in the field in tenths of its whole extent.

With an assistant for writing down the times of transit, if these are not chronographically registered, and the magnitudes, it would seem that all the stars of the Zone could generally be easily observed. For, with a mean number of 500 stars pro square degree, we would have to observe in the mean 8 stars pro minute in the equator; a smaller number in the higher declinations.

In the Milky way, where the number would be 3 or 4 times larger, the Zones would be taken, say, only of 2′ breadth or still narrower.

In this way the survey of a single area would generally require some two hours, in the Milky way double that time. A single Durchmusterung of the areas of both hemispheres together would thus require between 500 and 600 observing hours.

The precision of the positions need only be sufficient for identification.

With a rough but convenient method like that here described, errors greater than 1^{sec} and 0′4 [1]), or, in the Milky way, of 1^{sec} and 0′2 must be rare. This precision will probably be sufficient for the purpose. Doubtful cases will be revealed by the photographic work and may at once be cleared up.

For the reduction of the materials to a rational photometric scale, standards will be required.

As for stars of magnitude 9.0 or brighter such standards are already provided by the Harvard publications, these standards should be on the Harvard scale.

In my opinion 10 standards for each area, ranging from 10^{th} to 14^{th} magnitude, would be sufficient.

X. Photographic magnitudes.

The importance of photographic magnitudes along with the visual ones was briefly discussed in the introduction. If the difficultly of getting a rational scale can be overcome and if we can get homogeneity for the whole of the sky, we may certainly expect to get very precise determinations much more easily than we can obtain the visual estimates.

On Durchmusterung plates it will be easy to get rapidly the positions of all the stars, accurate to, say, 0′1, in both coordinates, together with an estimate of the diameter.

If we take only the stars within a circle of 41′5 radius (surface 1 5 square degrees) the images will be circular. They are so in most of the instruments now in use. It remains only to consider whether there is any prospect of overcoming the difficulties just now mentioned, I think these difficulties may be considered under three heads:

1st. How can the magnitudes from different parts of the same plate be reduced to homogeneity?

2nd. In what way can the magnitudes of the same plate be reduced to a rational scale, that is a scale having a constant light ratio between the stars of consecutive magnitudes?

3d. How can the plates in different parts of the sky be reduced to homogeneity?

In considering these questions I think there can be no difference of opinion but that our scale must be absolutely independent of any visual scale; only a shifting of the whole of the scale may be allowed, which will

---

1) If the power of the telescope allows the use of a faint field illumination (by red light?) five horizontal spider webs at intervals of 1′ would present considerable advantages, because it will give a precision of 0′1 in declination.

bring the two scales into agreement for the mean of all the stars of any one, arbitrarily chosen, magnitude.

In regard to the first point there can be no serious difficulty, at least if we may assume that the errors depending on the position on the plates may be considered to be the same for all the plates obtained with the same instrument. There may be cases in which this rule does not hold, but experience teaches that such cases are exceptional, especially when (as is here proposed) regions near the edges of the plates are avoided. If two plates of every area are taken, such exceptional cases will betray themselves by systematic divergences between the two and a repetition of the photograph will show which plate is unreliable.

If therefore we consider these errors the same for all the plates, we may certainly find out their value by comparison with the visual magnitudes or even by a comparison of the numbers of stars in different parts of the plates.

[No other dependence on visual magnitudes is introduced thereby than the assumption, that the difference of visual and photographic magnitude does not depend on the position on certain arbitrarily chosen plates, which assumption is, of course, absolutely safe].

Instances of such determinations may be found Cape Phot. Durchm. I pp. (23) — (28); or might be based on such an investigation as that of Turner, Monthly Not. Apr. 1902.

In this last paper the conclusion is reached that „as regards a uniform „scale of magnitudes over a large area, the doublet is immensely superior „to the single objective."

I think that the materials furnished by the Dm of Bonn, Cordoba and the Cape, will be sufficient for the purpose. We will obtain even more perfect materials as soon as the visual estimates for our selected areas shall be available. — Another method, not introducing the use of any visual magnitudes, is followed for the *carte du ciel* plates of Helsingfors. It consists in taking overlapping plates, the corner of which meet near the centre of the main plate.

Immensely more serious is the second difficulty.

It is with much hesitation that I venture to propose for trial the following scheme, which seems to hold out some promise of an issue out of it.

The impossibility in which I am of making anything but a preliminary trial myself must be my excuse, if the experiment proves unsuccessful.

The scheme assumes the use of certain neutral tinted screens of which the absorption is carefully determined. A series of photographs will have to be taken through these screens. Prof. Pickering has recently published (Ann. of Harv. obs. 48, p. 151) a result found by Mr. King, *viz.* that a

photographic film admirably realises a really neutral tinted medium, infinitely better than any shade glass.

Experiments made at Groningen, by Dr. de Sitter, prove further that these films can be easily made of such uniformity that the absorption in every part is the same within the errors of observation.

Now suppose a plate, half of which is thus blackened.

Let A be the transparent part, B the blackened part. — Suppose the absorption of light traversing the part A expressed in magnitudes to be ε (ε will be a very small quantity); that of light traversing B to be $\varepsilon + h$.

In order to get the magnitudes of stars in a determined region of the sky, the film of this screen will be brought into contact with a sensitive plate and a photograph of the region will be taken *through* the screen, of which suppose the blackened part B to be on the left hand side.

At the end of the exposure the screen will be turned through 180°, so that B will now be on the right hand side and another exposure of about the same duration will be made after having given a slight displacement to the telescope.

The plate will then be developed. Let S_1, S_2 and Σ_1, Σ_2 represent the images obtained by the two exposures of two stars S and Σ of nearly equal photographic brightness and situated, the one to the right, the other to the left of the middle of the plate.

Σ will have been covered by the blackened part of the screen in the 1st exposure, S in the second exposure, so that the images Σ_1 and S_2 will be much fainter than the images Σ_2 and S_1.

We will thus have something as follows:

Position of screen in 1st exposure $B\ A$,
„ „ „ „ 2nd „ $A\ B$.

Images of the two stars S and Σ on the developed plate:

Σ_1 •   ● S_1
Σ_2 ●   • S_2

Now, *even if the two halves A and B of the screen had been identical*, then still, on account of a change in the atmospheric conditions, of a small difference in the time of exposure etc., we might *not* expect perfect equality of the diameters S_2 and S_1 of Σ_2 and Σ_1, but we might certainly expect perfect equality in the *mean diameters*

$$(a) \ldots \frac{S_1 + \Sigma_2}{2} \quad \text{and} \quad (b) \ldots \frac{S_2 + \Sigma_1}{2}$$

because for both these quantities we have to do with identically the same *mean* atmospheric conditions, the same *mean* time of exposure etc.

In the real case, that of the two halves of the screen differing in their degree of transparency, the two quantities (a) and (b) will differ.

The difference will evidently exactly correspond to the excess of the absorption of B over A, that is to h *magnitudes*.

Therefore, if we assume m to represent the mean photographic magnitude of the two stars under consideration (we may take an arbitrary value for m) then on the plate

diameter $\frac{S_1 + \Sigma_2}{2}$ will correspond to magnitude m;

$\frac{S_2 + \Sigma_1}{2}$ „ „ „ „ $m + h$.

To extend the scale we may take two stars the mean diameter of which, as photographed through A, is $\frac{S_2 + \Sigma_1}{2}$ and which therefore are of mean magnitude $m + h$. The mean diameter of these same stars, as photographed through B, shall correspond with mag. $m + 2h$, and so on.

We thus get a scale extending over the whole range of magnitudes, the intervals of which are h magnitudes. We get new series, if we start with stars of about magnitude $m + \frac{1}{4}h$ and then with stars of about $m + \frac{1}{2}h$ etc.

It will thus become easy to derive some analytical or empirical curve for the reduction of all the diameters to magnitudes, if the interval h is not chosen *too* large, not larger than 2 magnitudes for instance.

There may be advantage in having the screen to consist of a series of alternately transparent and blackened sectors rather than of a transparent and blackened semi-circle, but this point need not be considered here.

In my opinion the question whether this plan will furnish reliable results, turns wholly on the question whether or not the enfeebled images of the bright stars have quite the same aspect as those of the *un*enfeebled images of the faint stars.

Some difference of aspect is to be feared, if the screen is placed at some distance in front of the sensitive plate, because the part of the blackened screen traversed by a pencil of light will itself become a radiator.

Experiments at Harvard observatory bear out this expectation. It is for this reason that the film of the screen ought to be placed in contact with the sensitive film. If a rapid deterioration of the screen is feared, then small pieces of tinfoil might be placed between the two at the borders.

At all events it will be necessary to use plates of plate glass.

Experiment must make out whether in this way equality of aspect is really obtained. A few experiments made by myself with artificial stars are favorable as far as they go. With our Durchmusterung apparatus, magnifying power 3.5, no difference of aspect of the images could be made out. Careful inspection under a microscope, power 24, however,

showed the images formed under the blackened part of the screen slightly more diffuse on the borders. — Taking the images slightly out of focus made even this difference disappear. In nearly every case, at least, I was quite unable to make out any difference.

But even the differences between the images well in focus are not greater than those which often occur between equally bright star images on one and the same plate. In addition to this we have to consider that any possible small outstanding constant error, caused by a slight difference in aspect, will be indifferent as long as the *same* observer makes the screen observations both of the real stars and those of the artificial stars for the determination of the absorption.

Prof. Pickering is making experiments on real stars.

The determination of the absorption h cannot cause any very serious difficulty. It ought certainly to be made *photographically* in *exactly* the conditions of the observations themselves. — We have made some experiments here, but it would too much extend the limits of this already over long paper, to explain these experiments here.

As a powerful check on the results of the screen observations, which can be obtained with very little trouble indeed, it would be advisable, along with the main exposures, to take a couple of additional exposures, with a time of exposure of, say, a *thirtieth* or a *sixtieth* part of the main exposure. Every bright star would thus get a couple of companions x magnitudes fainter. The quantity x might be derived from the whole of the accumulated screen plates and this quantity, once determined, would be a great help in establishing a rational scale.

By this means it may even prove sufficient to restrict the screen work to a small fraction of the whole number of the plates. There is the more reason for such a restriction, because it will certainly be necessary to compare the plates directly *inter se*, as I will now try to explain. I will assume, therefore, the screen observations limited to the 26 standard areas now to be considered.

XI. Homogeneity for different parts of the Sky.

If we succeed in thus obtaining a rational scale for the several plates, there still will remain a constant difference (index error) between them, because one of the magnitudes for every plate has been chosen arbitrarily.

In order to get homogeneity for the whole of the sky we have to determine these constant differences.

This question is doubtlessly the most important of all. Without a

satisfactory solution of it, the data for magnitude contained in such works as the *Cape Phot. Dm.* and the *Carte du Ciel* will be of little use for general cosmical investigations.

I see no other way of getting over it, than by direct comparison of our areas *inter se.*

As the matter is of so fundamental importance for all the observatories cooperating in the international work of the *Carte du Ciel*, we may hope that a certain number of them will not shun the very limited work, involved in some such plan as the following.

The fundament of the whole will be the rigid connecting of 26 of our 252 areas.

As the lower latitudes are the most favorable for the work, I will assume, for illustrations sake, that 6 observatories take part in this fundamental work.

2 at about latitude + 45°
1 " " " + 35
3 " " " — 35.

Alterations necessary in the case of observatories otherwise situated will be evident.

For the Fundamental areas let us choose:

8 Plates on the equator at RA $0^h.8$; 3.8; 6.8; 9.8; 12.6; 15.6; 18.6; 21.6.
8 " " " parallel of + 45, RA 0.6; 3.6; 6.6; 9.6; 12.8; 15.8; 18.2; 21.8.
8 " " " " " — 45, " 0.6; 3.6; 6.6; 9.6; 12.8; 15.8; 18.8; 21.8.
1 Plate at the North Pole.
1 " " " South Pole.

At every one of these observatories every comparison of two among the 26 areas will be made, which can be obtained by photographing the two areas on the same plate, at the same, moderate, zenith-distance.

We may thus obtain the following combinations under favorable circumstances:

$\varphi = 45°$.

Combinations.		Number of Plates.	Difference of R.A.	Zenith Dist.
Polar area with area at . . .	$\delta = 45°$	8	—	45°
" " " " " . . .	$\delta = 0$	8	—	45
Area at $\delta = 45$ with area at .	$\delta = 45$	8	3^h	16
" " " " " " " .	" "	8	6	32
" " " " " " " .	" "	8	9	46
" " " " " " " .	$\delta = 0$	8	6	48
" " " " " " " .	" "	8	6	48
" " $\delta = 0$ " " " .	$\delta = 0$	8	3	49
Total		64		

$\varphi = 35°$.

Combinations.	Number of Plates.	Difference of R.A.	Zenith Dist.
Polar area with area at . . . $\delta = 45°$	8	—	55
„ „ „ „ „ . . . . $\delta = 0$	8	—	55
Area at $\delta = 45°$ with area at . $\delta = 45$	8	3^h	20
„ „ „ „ „ „ „ . „ „	8	6	35
„ „ „ „ „ „ „ . „ „	8	9	51
„ „ „ „ „ „ „ . $\delta = 0$	8	6	45
„ „ „ „ „ „ „ . „ „	8	6	45
„ „ $\delta = 0$ „ „ „ . $\delta = 0$	8	3	41
„ „ „ „ „ „ „ . „ „	8	6	54
Total	72		

In the supposed case, therefore, we would have:

observatories at -35 producing each 72 plates	216 plates
observatory at $+35$ „ 72 „	72 „
observatories at $+45$ „ each 64 „	128 „
Total	416 plates

By these 416 plates we would have:

e area at the North Pole compared to 16 other areas, at 3 observatories,
h area at $\delta = +45°$ „ „ 9 „ „ „ 3 „
„ „ „ $\delta = 0$ „ „ 8 „ „ „ 6 „
„ „ „ $\delta = -45$ „ „ 9 „ „ „ 3 „
a at South Pole „ „ 16 „ „ „ 3 „

I find that in this way the difference between two regions will be termined with a weight of from 15 to 22 direct comparisons. A reliable termination of the differences required for 26 plates will thus be obtained.

It is desirable, though not absolutely necessary, that the times of posure should be so chosen as to give the faintest stars contained in r plan (mag. 14?).

The further work for obtaining a set of rational and homogeneous ignitudes for the whole of the areas might well be left to two observa-ies, one in each hemisphere.

The whole work would consist in:

a. Screen observations of the 26 standard areas;

b. 226 plates containing one exposure of any of our areas, together with an exposure of two standard areas. The main exposure to be taken, both in time and in Zenith-distance, about midway between the two standard areas. Additional short exposures very desirable.

On these plates only about 30 well chosen stars would be measured,

so that the result would be the determination of some 7500 star-magnitudes on a rigidly rational and uniform scale.

The determination of the magnitudes of the 200 000 stars would then present no further difficulties and would be made on the Durchmusterung plates.

The whole of the photographic work involved in obtaining the 7500 magnitude standards would thus, in my view, consist of (not counting the additional short exposures):

	Number of Plates.	Number of Exposures.
416 plates with 2 exposures (combination of 2 of 26 standard areas).	416	832
Triple set of 26 plates with 2 exposures through screen.	78	234
252 plates with 3 expos. (each area with 2 standard areas)	252	856
Total . . .	746	1922

At first sight such a work might perhaps seem too vast for the purpose. If however we consider that the work to be expended on the long exposure series of plates of the *Carte du Ciel*, on the plan followed in Paris, will require over 22 000 plates with 66 000 exposures of half an hour each, it will be conceded, I think, that, even if the plan outlined just now had no other aim than to furnish the means of reducing the *Carte du Ciel* to a uniform and rational scale, the work would be well worth the trouble. (It would be equivalent to an addition of some 40 plates to the number of plates to be taken by every observatory taking part in the *Carte du Ciel*).

XII. Proper motions.

Photography furnishes the means of obtaining numerous accurate proper motions in a relatively very short time. A method essentially the same as that used for the parallaxes is explained in Publ. No. 14 of the Astr. Lab. at Groningen. The plate after having received 2 or 3 exposures, is preserved undeveloped for a certain interval of time; after that time it is again exposed and then developed. The proper motion during the interval is readily to be derived from the measures of the distances between the different images. In the paper quoted a determination is made of the proper motions of the Hyades by means of plates the exposures for which were taken at intervals of partly *four*, partly *five* years.

It appears that with 3 or 4 of such plates we get already an accuracy equal to that of the p. m. of Auwers'-Bradley depending on 3 obs. of

Bradley. — In this case we had a number of overlapping plates, which made it possible to make the reduction depend on a very large number of well determined standard proper motions. In the Preface (pp. VIII—XII) I tried to show how, with an interval of 10 years, even a single plate of the *Carte du Ciel* type, with 6 exposures, may well be made to yield proper motions with a probable error below 0″01.

It will be necessary for that purpose to have a dozen A. G. C. stars reobserved on the meridian about 1915 in order to provide us with the necessary standard proper motions.

In the present case, where the stars will be very numerous, it seems safe to reduce the number of images to four and to double the number of plates.

I am now convinced that the method, as developed in the preface to the Groningen Publ. No. 14, is not the best for the present plan.

Let Δx and Δy represent the distance in x and y of the images of a star at the two extreme epochs Γ_1 and Γ_2; μ_x, μ_y the components of the proper motion, then we may assume (see the paper quoted) that

$$(1) \quad \ldots\ldots \quad \begin{cases} \mu_x = \dfrac{\Delta x}{\Gamma_2 - \Gamma_1} + a + bx + cy \\[2ex] \mu_y = \dfrac{\Delta y}{\Gamma_2 - \Gamma_1} + A + Bx + Cy. \end{cases}$$

In Publ. 14 the proposal was made to derive the 6 constants a, b, c A, B, C from the known proper motions of a dozen standard stars on each plate.

A plan better adapted to the present scheme, will be to assume that for the very faint stars on the plate, (after having excluded those among them, which by a first reduction appear to have a very sensible p. m.) the mean proper motion both in α and δ (μ_x and μ_y) *is the same over the whole of the plate.*

Starting from this supposition, the whole of these faint stars will at once furnish us with very reliable values of the four constants b, c, B, C. The two remaining ones only (a and A) shall be derived from the standard proper motions determined at the meridian instruments.

The following remarks might be made with regard to the whole process:

1st. The supposition here made will introduce a very slight systematic error because the parallactic motion will not be absolutely the same on every part of the plate. With areas the size of those here under consideration the error will never reach a *fortieth* part of the whole of the parallactic motion, however. This whole amount will, almost certainly, not

reach 1″0 in a century, the *fortieth* part of which may be regarded as quite insignificant.

Moreover, at the end of the work, when the mean parallactic motion of the stars of the 13th and 14th magnitude will have been determined, an *à posteriori* correction might be applied if deemed desirable.

2nd. The reason why the present process seems preferable is, that we thus have always a very considerable number of stars for the determination of the constants b, c, B, C, whereas the accidental errors of our assumption will, as a rule, not be greater than the accidental errors of the standard p. m.

3d. The prob. err. of the final p. m. will become very considerably smaller in this way; in fact with, say, 10 standards on a plate and a difference in epoch of 10 years, I think that we may be *sure* of getting the individual p. m. with prob. err. not exceeding 0″.01 in each coordinate; probably the accuracy will be greater.

4th. In many cases, especially if the field available is not 2° × 2° as in the *carte du ciel* instruments, it will be often impossible to get 10 standards on some of the plates [1]). In this case even *four* good standard p. m. will allow us to get good p. m. by the present process.

5th. In the few cases that even this number is not available, the best plan will probably be, to improve the value of the constants a and A by the assumption that the mean p. m. in x and y for the very faintest stars (excluding those with distinctly appreciable motion), is equal to that of the surrounding plates.

6th. For getting reliable standard p. m. it will be necessary to obtain meridian observations at about the epoch 1915. For the Northern Hemisphere these will already furnish good p. m. if combined with the positions of the A. G. C.

For the Southern Hemisphere we will in great part have to rely on Goulds Catalogues. The discussion of Gill, in the preface to his catalogue of standard stars for astrographic plates (now in print), proves the necessity of a very careful preliminary investigation of the systematic errors of these catalogues.

7th. These determinations will be much strengthened by all the older observations, which in 1915 will probably be available in the most convenient and reliable form by the labour for the *Geschichte der Fixsterne*.

---

1) In estimating what we may hope to get, use may be made of the following numbers:

On an average:

the A. G. C.	contains	6	stars pro square degree;
Goulds Gen.-Cat.	„	2.6	„ „ „ „
„ Zone-Cat.	„	6.6	„ „ „ „

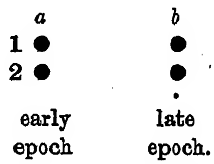
8th. As the accuracy of Goulds Zone catalogue is inferior to that of the A. G. C. and the number of stars in the General catalogue is, relatively speaking, small, it will be desirable that, at least in the Southern Hemisphere, the field be not below $2° \times 2°$.

9th. With such a field we may hope to get a satisfactory number of standard p. m. even in the Southern Hemisphere. — For the Zone —15° and even to a certain extent for Zone —30°, there are available, for strengthening the p. m. obtained by the aid of Goulds catalogues, a number of older observations obtained at the Northern observatories (Lalande, Bessel, Lamont, Argelander, Washington Zones, Schjellerup, Santini).

For Zone —45°, Gills standards, observed about 1898, will already powerfully contribute. For Zone —75 and the polar plate, we have Gilliss's Zone observations.

For the rest, the catalogues of Piazzi, Taylor and Stone must have a considerable number of stars on the area of the plates.

10th. It must be considered essential that the images at the two extreme epochs get very near each other on the plates and that they are as nearly equal in diameter and density as it is possible to get them. Let the images be as follows: 1)



The distance *ab* ought to be nearly the same for all the stars on the plate and not exceed, say, one millimeter.

With an interval between the epochs of 10 years, this condition might easily give rise to some anxiety on the part of the observer, because alterations in the instrument by which the relative position of the photographic and the guiding telescope might be changed, may easily occur or become necessary in so long a period.

The difficulty can be easily overcome, however, if, on the same nights with the first exposures, some *check plates* are taken; I mean plates exposed just like the main plates but only during, say, half a minute; which plates are then stowed away, undeveloped, with the others.

Now, before taking the exposures for the later epoch, these *check plates* will be first exposed, again for half a minute, and they will then be immediately developed. Simple inspection will then show at once

1) The small additional image must serve the purpose of preventing any mistake in the chronology of the images.

whether the images get in the right position and whether or not they are equally bright with those of ten years ago.

If not then they will indicate what corrections in position and time of exposure will be necessary.

Some of these *check plates* further will enable the observer to convince himself from time to time whether the plates keep sufficiently well.

It will certainly be desirable to make the exposures at the two epochs both on the meridian or in the same small hour angle. If we do this the only systematic error that need be feared in the proper motions is an error dependent on the magnitude.

By defects in the guiding the images of the brighter and fainter stars may be affected in a somewhat different way. If so, then the p. m. of the faint stars on the plate, depending as they do on the proper motions of standard stars of magnitude 9.0 or brighter, will be systematically in error.

The same errors will be discussed in some detail in connection with the parallaxes.

For the proper motions, a good way to get practically rid of them may be as follows:

For every area the proper motion of some 4 stars of the 11th and 4 stars of the 13d or 14th magnitude relative to as many stars of equal brightness with the standards will be determined, using all the materials furnished by the whole of our plan.

They will be chosen so near to the bright stars that an error of distortion of the film need not be feared.

For this purpose our plan will furnish:

A. For the early epoch *E*: [1])

p. m. plates: 2 plates with a total of 4 exposures (series No. 5);
parall. „ 2 „ „ „ „ „ 16 „ („ „ 6).

B. For the late epoch *L*:

p. m. plates: 2 plates with a total of 4 exposures (same as for epoch *E*, series No. 5)
parall. plates: 1 plate with a total of 8 exposures (series No. 7).

These supplementary measurements need give very little trouble, if they are made during the time that the plates lie on the measuring machine, carefully oriented, for other purposes.

The number of the exposures is so great that we will easily recognise any cases of considerable magnitude error. Excluding these we will get results from the remaining ones, in which but little of this error can be left.

---

[1]) See Recapitulation further below.

The proper motion of the selected stars being once known, we will be enabled to correct the plates for the error in question in a satisfactory manner.

It is not necessary to restrict ourselves wholly to the standard p. m. of those stars for which we will have meridian observations about 1915.

With 10—12 stars observed in the meridian for each plate the plates themselves will furnish accurate modern positions for any stars of which we possess older observations.

The advantages of the method of deriving the proper motions from star images on one and the same plate over that which would derive them from a comparison of different plates, are the same as for the parallaxes and have been discussed in Groningen Publ. No. 1. For the p. m. a more direct comparison of the merits of the two methods has been set forth in the Preface to Publ. No. 14, p. VII—VIII.

The advantages are such that, in order to obtain good proper motions *in a short time*, the first method is incontestably far superior.

After the lapse of years however, it is evident that these proper motions can be improved by taking new plates to be compared with older ones.

For this purpose the long exposure plates now being taken for the *Carte du Ciel* will serve admirably, if those among them which coincide with our areas are repeated at some future time. — Several of these plates must have been actually taken. I venture to suggest that to those not yet taken a certain degree of precedence may be allowed in order that their epochs may be as early as possible.

XIII. Parallaxes.

In the present state of science we cannot hope to determine the individual parallaxes, accurate to a small fraction of their amount, for any great part of the stars. What we may hope to attain is such a determination for the *mean* parallax of numerous groups, as for instance the mean parallax of:

(1) stars of different magnitude;
(2) stars of different proper motion;
(3) stars of different spectrum;
(4) stars for which the value: phot.-vis. mag. has a determined amount;
(5) starclusters, etc.

For some of these groups, as (1), (3), (4), these mean parallaxes may be got also, and with smaller prob. err., from the parallactic motion. Still for these too a direct determination is wanted as a check, because

the results obtained by the parallactic motion depend to a certain degree on the hypothesis made about the distribution of the real motions of the stars.

The mean parallaxes sought are so small and the number of stars to be dealt with is so great, that only a very expeditive method, with the most absolute guarantee against systematic error can be applied with any chance of success.

A method which seems fairly to meet the requirements of the case was proposed in Publ. I of the Lab. of Groningen, to which I must here refer.

In accordance with that method we will suppose that each plate contains 8 images of every star, and three additional ones (requiring a few seconds of exposure) of the brighter stars:

Thus:

1st Max. 2nd Max. 3d Max.

Two of the main images belong to the 1st Max: 4 to the second: 2 to the 3d. The additional images of the brighter stars, serve the purpose of enabling us afterwards to distinguish very conveniently and with certainty between the different maxima.

Remarks:

1st. For the parallax work the introduction of *check plates* may also powerfully contribute in relieving the observers mind of much anxiety. About these plates see p. 36.

2nd. It might be advantageous, especially in an unreliable climate, not to take more than *four* images on each plate and then to double the number of plates. This will not prevent us from making the reductions at once for the *mean* of the several images.

If all the exposures on any plate are made in the meridian or at least in the same small hour angle, systematic error seems impossible as long as care is taken:

a. that the stars of comparison are well distributed among the stars of the group to be measured;

b. that the stars of comparison are of equal magnitude with those of the group the mean parallax of which is sought.

Both conditions are quite easily satisfied in the investigation of such groups as (2), (3), (4).

For such a group as (1) condition (*b*) is evidently not satisfied. Now, however, even a difference of magnitude can only give rise to a *semi-systematic* error, viz: to an error which, though systematic for a whole plate, must vary in amount and direction for different plates.

This error is caused by accidental defects in the guiding which, in some cases, must displace the images of the brighter and fainter stars in an unequal degree.

If the determinations of the mean parallax of stars of different magnitude were made dependent on many thousands of plates, then even this source of error would be eliminated. With a number of 678 plates they will doubtlessly become very small. As traces of it might however remain, the case calls for a closer examination.

Happily plates having appreciable magnitude error make themselves at once suspect.

If for any one plate we find the parallaxes of the brighter stars smaller than those of the fainter ones there will be almost certainty of such error. If we find them greater, but considerably more so than is found for the mean of all such stars, they again become suspect.

In most of these cases the suspicion will almost become a certainty by a comparison with the results of the duplicate plate of the same area.

For plates on which the different exposures have been taken in very different hour-angles such magnitude errors are often found. In most cases they are undoubtedly of the class of the hour-angle errors, however. (About this sort of error see Publ. of the Groningen Lab. No. 1, pp. 72—76 and No. 10 pp. 30—32).

With exposures all taken in the meridian, or at least in the same hour-angle, they must be far less frequent.

There thus cannot be any great obstacle against repeating all suspicious plates [1]).

Further below it will be proposed that a *double* set of parallax plates be taken at about the time of the first exposures for p. m.; a *single* set 10 years later.

The first double set will doubtlessly be measured and discussed before the 3d set is taken.

(A) For this double set a good rule would seem to be: to repeat all the plates where the mean parallax of, say, the 12 brightest stars on the plate, relative to the faintest stars, is found to differ as much as $0''.05$ on the two plates of the same area.

For cases where there is a suspicion of a nearly equal error in the same direction for both plates, the double set of images on each plate will enable us to derive and discuss *four* separate sets of results. These

[1]) The plates found to be affected by magnitude error will of course keep their value in investigations of such groups as (2), (3), (4) . . . . .

would no doubt be sufficient in nearly every case to decide whether the suspicion was well founded or not.

For the very small errors of this sort which would pass unnoticed, any tendency to a truly systematic influence, slowly varying with the time, (caused by a tendency of the observer to make the same guiding error on several consecutive days) can be most effectively rendered harmless by alternating as often as possible the exposures of regions of maximum and minimum parallax (that is by alternating morning and evening observations).

With all these precautions it must be admitted as certain that the supposition of a plate error corresponding to a prob. err. of $\tau = \pm 0''02$ for the parallaxes of the stars 9.0 relative to those of the magnitude 13.5, is rather extreme.

Farther below I will start from this supposition.

For the magnitudes: 7.5; 8.5; 9.5; 10.5; 11.5; 12.5; 13.5 will be assumed the values $(\tau) \pm$ 0.027; 0.022; 0.018; 0.013; 0.009; 0.004; 0.000.

Meanwhile there must remain a considerable loss in accuracy, which, it is true, will not prevent us from getting results accurate within a relatively small fraction of the amount, but which still will make an independent check more desirable than ever. Such a check would be of especial value if absolutely independent of any magnitude error.

The following method satisfies this condition.

The difficulty arising from the magnitude errors being the more serious, the brighter the stars, let us suppose the mean parallax of the stars of the 9th magnitude to be sought.

As our plates can only give *relative* parallaxes whereas, for purposes of cosmical investigation, we want *absolute* parallaxes, it is evident that we have to borrow something from other sources. We may, for instance, determine the mean parallax of the stars of magnitude 13.5 by their parallactic motion and derive from the plates the parallax of the stars of magnitude 9 relative to those of magnitude 13.5.

This however introduces in full any outstanding trace of the magnitude error.

Instead of the absolute parallax of the magnitude 13.5 we may however borrow from the parallactic motion the value of

$$(1) \quad . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \quad P = \frac{\pi_{II}}{\pi_I}$$

where π_{II} denotes the mean absolute parallax of the stars of the 9th magnitude and of the 2nd spectral type; π_I the same quantity for type I.

From the plates, on the other hand, we can get a determination of the quantity

$$(2) \quad \ldots\ldots\ldots\ldots \quad Q = \pi_{II} - \pi_{I}$$

absolutely free from any magnitude error, indeed not conceivably liable to any systematic or semi-systematic error whatever.

The combination of the two results gives:

$$(3) \quad \ldots\ldots \quad \pi_{I} = \frac{Q}{P-1} \qquad \pi_{II} = \frac{PQ}{P-1}.$$

As there will be found probably an approximately equal number of stars of the two types and a relatively insignificant number of other types, the whole mean parallax π will be, roughly:

$$(4) \quad \ldots\ldots\ldots\ldots \quad \pi = \frac{1}{2}\frac{P+1}{P-1}Q.$$

We thus get the absolute parallax free from magnitude error. For brevity this method will be referred to as Method *B*, whereas the method of measuring directly relative parallaxes of the stars of two different magnitudes will be called Method *A*.

The precision, in regard to accidental error, of the two methods will appear from the table further on. In computing that of Method *B* I have assumed, that the value $P = 2.3$, found in Publ. No. 8 from the observations of the bright stars down to magnitude 6.5, for which it proves to be practically a constant, will also be found to hold for the fainter stars.

For the stars too faint to allow of a determination of the class of spectrum the quantities:

(5) *Photogr. magn. — visual magn.*, may probably render us an analogous service.

The stars for which the difference is decidedly positive will in great part belong to the first type, whereas those for which it is decidedly negative will be mostly of the second.

Possibly a slight modification might be made of this method which would be still less open to objection.

Suppose that we find the distribution of the proper motions of the stars of type I, magnitude 9.0, and those of the stars of type II, magnitude $9.0 + h$, perfectly alike both in direction and magnitude, as it seems highly probable that we will, for some value of h near 2 magnitudes. We will reasonably be compelled to conclude that the stars of these two classes have no systematic motion relative to each other and the equality of their parallactic motions will in this case prove, practically independently of any hypothesis, their equality of distance.

Their relative parallax must therefore be found *zero* on the plates. If, by actual measurement, we find another value, this will be the magnitude error between the magnitudes 9.0 and $9.0 + h$. We will thus be enabled to determine this error and correct our results for it.

It implies the knowledge of the class of spectrum down to magnitude $9.0 + h$, or rather down to the faintest stars on the plates, if we refuse to assume that the error is proportional to the difference of magnitude.

If, however, we find that the quantities (5) can really take the place of the spectra in this investigation, the present modification of the method would seem to promise well.

We have an example here of the many advantages which will be gained by having the determination of *parallax*, *proper motion*, *spectrum visual* and *photographic magnitude* all made for *the same* stars.

Having thus cleared the way, I will now try to show in how far we may expect the execution of the present plan to furnish us with mean parallaxes which will sufficiently exceed their probable errors.

For this purpose I will set down the best estimates for the value of the mean parallax of certain groups of stars, which I find it possible to make; together with the probable errors with which we may hope to determine them. I could only give the groups of determined magnitude and proper motion, because data for other groups are too scanty for making plausible estimates.

In evaluating the probable errors I start from the assumption, already alluded to just now, that of each area three parallax photographs will be taken with 8 exposures each (not counting the faint additional images).

The precision which was got from plates taken at Helsingfors with a telescope of the *Carte du Ciel* type has been discussed in our Publ. Nos. 1 and 10. Afterwards Prof. Küstner of Bonn had the kindness to take another series of plates, all photographed *in the meridian*, with a telescope having $1\frac{1}{2}$ times the focal lenght of that of Helsingfors.

The measurement of these Bonn plates show the enormous advantage of remaining in the meridian, even in regard to accidental error. This need be no cause of surprise. For, the images being all taken in identical conditions, are far more uniform. The smaller Zenith-distance too is a decided advantage.

From seven of these plates I obtained for the quantity which in Publ. No. 10 was denoted by ϱ_1', and which represents the prob. error of a parallax based on 4 images on a single plate, in the case of a parallax coefficient *unity:*

(6) . . . . $\varrho_1' = 0''025$

just half of that found in Publ. No. 10 for the shorter focal length and exposures in different hour-angles.

It is true that this determination is somewhat weak owing to the small number of stars on the plates. Recently Prof. Küstner himself has investigated the parallax of Nova Persei. The measurement of 60 stars on eight plates gave the value, well guaranteed this time, of

$$\varrho_1' = 0''020.$$

(For a preliminary notice about these measures see: Vierteljahrschr. der Astr. Gesellsch. 40 (1905) p. 101. For the number given just now however see Prof. Küstner's letter in the Appendix).

Still this astonishingly low value must be an upper limit, because it assumes the real parallax of all the stars measured to be zero. In fact it is merely the probable amount of the values found for the parallaxes relative to the mean of all the stars on the plates. — In order to be quite certain of not overestimating the accuracy, I will, in what follows start from the value (6).

From this value it follows that, with a mean parallaxfactor of 0.80, which we will presently find to be attainable, *three* plates, each with eight images, will give the individual parallaxes with probable errors of

$$(7) \quad \ldots\ldots\ldots\ldots \quad \pm 0''0128.$$

This represents the probable error of the parallax of a star relative to the mean of all the stars on the plate.

The probable error of the mean of n stars relative to the mean of n' stars of comparison will be practically:

$$(8) \quad \ldots\ldots\ldots \quad \pm 0''0128 \sqrt{\frac{1}{n} + \frac{1}{n'}}.$$

With longer focal length we may be sure of still considerably improving on this value. It will be safer, however, to neglect this consideration for the present and stick to numbers already actually obtained.

As it must be obvious that an enormous amount of labour would be wasted if, with the possibility of attaining to such a precision, we still used plates of a lower accuracy, I think we must certainly conclude: that no instrument ought to be used for such extensive plans as the present one, which does not allow of easily obtaining for the individual parallaxes a precision corresponding to a probable error of $\pm$ 0″02 *at least*. As far as can be judged at present the focal length ought not to fall below 5 metres for this reason.

From our data we get to following results:

Magnitude groups.

Mean Mag.	$n =$ numb. of stars.	Estimated mean π	Method A. P.E. of parallaxes rel. to mag. 13.5 — exclud. mag. error $0''0128\sqrt{\frac{1}{n}+\frac{1}{4300}}$	Method A. P.E. of parallaxes rel. to mag. 13.5 — includ. mag. error $\sqrt{(0.0128)^2\left(\frac{1}{n}+\frac{1}{4300}\right)+\frac{\tau^2}{678}}$	Method B. P.E. of absol. π $\frac{0''0128}{2}\frac{P+1}{P-1}\frac{2}{\sqrt{n}}$	P.E. in fraction of whole — Meth. A includ. mag. err.	P.E. in fraction of whole — Meth. B.
7.5	200	0″0090	0″00092	0″00139	0″00230	0.15	0.26
8.5	600	.0068	.00056	.00101	.00133	0.15	0.20
9.5	2000	.0053	.00035	.00077	.00073	0.14	0.14
10.5	4300	.0043	.00028	.00057	.00050	0.13	0.12
11.5	4300	.0034	.00028	.00044	.00050	0.13	0.15
12.5	4300	.0026⁵	.00028	.00032	.00050	0.12	0.19
13.5	4300	.0021	—	—	.00050	—	0.24

The estimated parallaxes were taken, for the brighter stars from Gron. Publ. No. 8; for the fainter ones they were still somewhat improved by considerations based on the results of Publ. No. 11.

Proper Motion groups.

Proper motions.	$n =$ estimated numb. of stars.	estimated absolute π	π relative to stars of equal mag. and insensible p.m.	Prob. err. $= \frac{0.0128}{\sqrt{n}}$	Prob. err. in fract. of whole.
0″05—0″10	1800	0″004?	0″002?	0″0003	0.15?
0.10— 0.15	290	.012	.010	.0007⁵	0.07⁵
0.15— 0.20	130	.015	.013	.0011	0.08⁵
0.20— 0.30	40	.019	.017	.0020	0.12
$>$ 0.30	30	.033	.031	.0023	0.07⁵

The estimated numbers in the 2nd column were obtained, by extrapolation, from the table of Publ. No. 11, p. 8.

The estimated parallaxes from Publ. No. 8.

The mean parallaxes of the several magnitudes are thus seen to be determined by Method A (including magnitude error) with a p.e. of about 14 percent of their amount.

There thus can be no doubt but that we must be led to reliable results.

As was to be expected the numbers for the p. m. groups look still more promising. For groups like (3) and (4) this must be the case even

in a higher degree. I thus conclude that a few years of labour could hardly be better bestowed than on a trial of getting such and suchlike determinations, which are fundamental for cosmical investigations.

In Publ. No. 1 the best conditions for obtaining good parallax plates were summarized as follows: (For the present purpose condition *c* can be omitted).

(*a*) All the exposures to be made on the meridian or in a constant (small) hour-angle for all the maxima.

(*b*) In the making of the photographs, regions of maximum are to be alternated as often as possible with regions of minimum parallax.

(*d*) Every plate is to be measured in its *two* positions.

Of these I consider the last the least important. In an extensive plan like the present, it will probably turn out that, with the exception of those plates which show very sensibly different images for the several maxima, measurement in one position will be sufficient. Frequent alternation of this position for different plates will be recommendable (so that, whatever traces of personal error might still attach to the results of single plates, will be surely eliminated in the mean of several plates.)

If we find the case to be really as here supposed, an enormous time will be saved in this way, without material loss of accuracy.

The condition (*a*) will make it impossible to get all the exposures exactly at the maximum of parallax.

For the observatories in high Northern latitudes the short summer nights will be cause that the regions in right ascensions between 12^h—24^h cannot be obtained in advantageous circumstances. The necessity of morning observations is especially troublesome in situations where the weather is very variable.

For these reasons the observatories best situated for parallax work are certainly those at low latitudes. In Publ. No. 15 of the Groningen Laboratory DE SITTER has tabulated, for different latitudes, the limits of the time during which stars of different *RA* can be photographed on the meridian, with a depression of the sun of 12 degrees or more. The best factors obtainable between these limits are also given and further the mean factor during the month following the 1st date of the observation-period and that during the month preceding the end thereof.

I find that at $\varphi = 40°$, the best obtainable coefficient is in the mean: 0.88; the coeff. during the months limiting the observation period: 0.78.

For lower latitudes these coefficients become still somewhat more favourable. (See summary on p. 11 of Publ. No. 15.)

For these reasons and those given above, I think that to conditions (*a*) (*b*) (*d*) we must join, for the present plan:

(*e*) Focal length of telescope considerable; if anyways possible not below 5 metres;

(*f*) Cooperation of two observatories in latitudes between ± 45 and the equator essential to the success of the plan.

As the field of the telescopes of long focus is usually not very extensive, it might be useful to choose the stars to be measured for parallax, and consequently for proper motion, in a small area.

I do not think however that it would be desirable to place the limit lower than half a square degree, mainly because the number of bright stars would become too small.

XIV. Class of Spectrum.

Already with the Bache telescope, provided with an objective prism (Aperture 8 inches), the spectra have been obtained of all the stars of the whole sky down to magnitude 8 (see Ann. of Harv. Coll. obs. vol. 26 part. I, pp. XII and XIII).

There thus would seem to be good ground for the hope that, for the very small part of the sky covered by our plates, it will be found practicable to get the stars of magnitude 9.0 or even 9.5.

The importance of such a determination appears from such considerations as given before, in discussing the importance of the class of spectrum and again from the means it will create for overcoming the difficulties caused by remaining magnitude errors in the parallaxes.

The spectra will of course be classified for the whole of the stars impressed on the plates, whatever their extent may be, thus furnishing fairly ample materials for the discussion of the distribution of the spectra of the fainter stars. — Of special importance however will be those of the stars of which the proper motion and the parallax will have been determined.

XV. Radial velocities.

The possibility of getting spectra of faint stars, of sufficient dispersion to give fairly accurate determinations of radial velocity, appears from the experience gained at the Lick Observatory in measuring the radial velocity of 1830 Groombr. Prof. Campbell states (Bull. Lick Obs. I p. 25) „the greatest „interest of the observations lies in the fact that fairly accurate determi- „nations of stellar velocities are shown to be possible down to the eighth „or ninth photographic magnitude, provided the spectra contain well „defined lines."

Of late no more hopeful word has probably been spoken for those interested in the future of the sidereal problem.

Already 700 stars appear to have been photographed for radial velocity by the Lick observers. This is certainly more than could have been hoped for a few years ago.

Still it is evident what an enormous labour will be involved if, by the present methods, we wish to obtain determinations for faint stars, requiring long exposures, on anything like the scale on which we have data for astronomical proper motions.

It is because of this consideration and of the exceeding importance of the matter for a plan like the present that, notwithstanding my inexperience in the matter, I venture to direct once more the attention to other methods which, if some evident difficulties could be well overcome, would yield great numbers of determinations in a relatively short time.

The accuracy of the methods at present in use, has become so great that, wholesale results, even if far less accurate, would still be invaluable.

As far is I know, two promising methods, of the sort here alluded to, have been proposed [1]. Both assume the use of an objective prism, which has the double advantage of using the light of the stars to better advantage and of giving a great many spectra at the same time.

The first is that of Orbinsky, explained Astron. Nachr. Vol. 138, p. 10. It is based on the fact that in the spectra obtained by prisms (for gratings the effect would be far smaller) the displacement of the lines, due to radial velocity, is very different in the different parts of the spectrum.

The consequence is that the distance of two lines, somewhat wide apart in the spectrum, is changed by radial velocity. This change of distance can evidently be used as a measure of the velocities. That it is a tolerably *sensitive* measure appears from the fact that, according to Prof. Vogel, the variation in the distance of two lines at $\lambda = 410$ and $\lambda = 486$ will amount to 58 percent of the total displacement of the line $H\gamma$, for the spectograph of Potsdam.

The method evidently requires the use of a standard star (perhaps two), the radial velocity of which has been obtained by another method.

One would expect that the loss in the amount of the quantity to be measured will be more or less compensated by its purely differential character. It is not easy to see how, with moderate care of the observer, errors of a systematic kind could enter into the results.

---

[1] As this paper is going through the press Prof. Comstock proposes still another method in the Astrophysical Journal.

Prof. Vogel has given expression to his estimate of the method in these words: (l. c. p. 14).

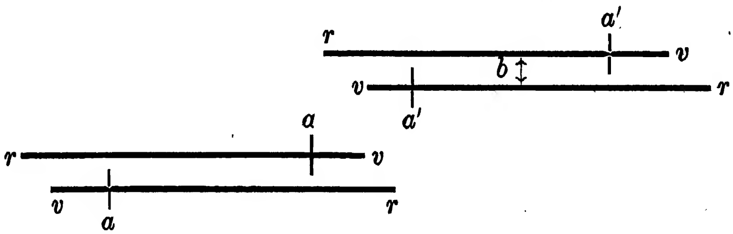
„Es unterliegt keinem Zweifel dass die angegebene Methode in den „Händen eines geschickten und umsichtigen Beobachters Resultate geben „kann, deren Genauigkeit mit den neueren Bestimmungen der Geschwin-„digkeit der Sterne im Visionsradius vergleichbar ist. Sie gewährt den „grossen Vortheil, nunmehr auch das lichtstarke Objectivprisma zu Be-„wegungsbestimmungen im Visionsradius anwenden zu können, welches „bisher im Allgemeinen nur Bilder der Sternspectra geben konnte."

The other method (Method II) is that proposed by Prof. Edw. C. Pickering in Astron. Nachr. Vol. 142, p. 105.

Slightly modified for the purposes of the present plan it comes to this:

A photograph of the spectra of the stars of a certain region of the sky having been taken, let the prism be rotated through 180° and let the position of the telescope be so altered that a second exposure, on the same plate, will give spectra quite near to those of the first exposure, but with the shorter wave length turned the other way.

Thus:



For two stars *very near together*, the distance *aa* ad *a'a'* of the same lines in the two corresponding spectra will be the same if the two stars have the same radial velocity.

If not, then the difference will represent *double* the displacement of the line corresponding to the difference of the radial velocities. If therefore the radial velocity of one star is known, that of the other will be found by measuring both the spectra.

For a reliable reduction of stars not lying very close together — consequently for the reduction of the whole of the spectra on the plate — even *two* stars with known radial velocity will be required.

Of course a somewhat greater number of standard stars must materially improve the results. — Moreover a few photographs of regions with a greater number of standards [1]), or better perhaps, of a field with arti-

[1]) The best field for a preliminary trial is probably in the Hyades, because there are a great number of stars in this region, which undoubtely belong to a physical group

ficial stars will be desirable in order to determine certain parameters, which are practically constant for the different plates obtained with one and the same instrument [1]).

Finally: both the methods suppose *the same* lines measured in all the spectra. There will be a difficulty in satisfying this condition, especially for stars of different type. I feel incompetent to decide whether this difficulty can be satisfactorily overcome by any other method than by adding another standard star differing in type from the two standards assumed above.

Instead of turning the prism, we can also pass from one position of the telescope to the other, if at the same time we turn the plate through 180°.

In both cases the quantity here measured is *double* that in the visual method and 3 or 4 times that in Orbinsky's method.

On the other hand it seems both less convenient and less independent of different sources of error than the latter. The choice between the two must be settled by experiment.

Meanwhile it is evident that the exposures obtained in two positions of the prism, allow the application of *both* the methods.

Now that Prof. Campbell thinks it possible to measure fairly accurately the radial velocities of stars of the 8th or 9th magnitude (photographically), we may certainly hope that, on powerful instruments, the objective prism will produce fairly satisfactory spectra of stars down to magnitude 9.0.

On plates of a useful surface of

$$4^\circ \times 4^\circ$$

the mean number of stars down to that magnitude, according to Pickering, is slightly over 50.

Even if half only of these spectra show well measurable lines, our 252 areas would thus furnish us with the radial velicity of some

6300 stars.

---

and must have nearly *equal* radial velocity.

With a field showing good images over an area of

$$2^\circ \times 2^\circ$$

and the centre at

$$\alpha_{1855} = 4^h 18^m 30^s \qquad \delta_{1855} = +\ 15^\circ\ 29'$$

we would have on the plate (see Publ. Astr. Lab. Gron. 14, p. 87), belonging to the physical group:

	very probably	probably	doubtfully
8.4 or brighter	14 stars	2 stars	4 stars
8.0 „ „	13	2	3
7.0 „ „	8	1	1

For a field of only $1\frac{1}{2} \times 1\frac{1}{2}$ the centre at $4^h 20^m 30^s + 15^\circ 9'$ would be preferable.

[1]) For somewhat more ample details see the concluding remarks.

For a fraction of these, all the other elements: photographic and visual magnitude, proper motion and parallax, would also become known; for these stars all the data obtainable would thus be practically complete.

The question naturally arises whether the determination by the usual methods, of the radial velocity for a few of the brightest stars of each area, which must serve the purpose of standards, is not better deferred till experiments shall have shown the real capabilities of the wholesale methods. I think not. — For even if the wholesale methods fail to produce reliable results, the knowledge of the radial velocity of 3 or 4 stars, of about the 7th magnitude, for each area, that is of a total of 700—900 stars, well distributed over the whole of the sky, will evidently be an invaluable addition to our data. Particularly so because of the discovery of Campbell, already referred to before, of the exceptional character of the radial velocities of the very bright stars.

XVI. Brightness of the sky.

On this head nothing need be added to what has already been said.

XVII. Recapitulation of the whole labour.

A. **Photographic** (additional short exposures not counted).

		Number of plates.	Number of exposures.
1.	*Durchmusterung.* A complete set of 252 plates of long exposure (with two additional short exposures . . .	252	252
2.	*Photographic Mag.* 416 plates connecting 2 of 26 standard areas	416	832
3.	„ „ Triple set of 26 stand. areas with 2 expos., through screen	78	156
4.	„ „ 226 plates for comparing each area with two stand. areas	226	678
5.	*Proper motions.* Double set of plates with two early exposures, two ten years later	466	1864
6.	*Parallax.* Double set of plates with 2 exposures at first, 4 at second, 2 at last maximum	452	3616
7.	„ Single set of plates like preceding, to be taken about time of last exposures for p. m. . .	226	1808
8.	*Class of Spectrum.* Double set	504	504
9a.	*Radial velocities.* 3 or 4 standards pro area?	?	?
9b.	„ „ Objective prism plates.	252?	504?

Totals.	Number of Plates.	Number of exposures.
Durchmusterung	252	252
Photogr. Magnitudess . . .	720	1666
Proper motion	466	1864
Parallax	678	5424
Class of Spectrum	504	504
Radial velocity	?	?
	2620 + ?	9710 + ?

B. Visual Observations.

Visual magnitudes.	Number of Stars.
10. Photometric observ. of 12 standards for each area. . .	3 024
11. Estimates and rough positions (Durchmusterung) . .	200 000
Proper motions.	
12. Observation in Meridian of 10—12 standards for the 233 p. m. plates	2 563
13. Brightness of sky	—

C. Measuring.

Photographic magnitudes etc.	Total number of images to be measured.
14. Duplicate Durchmusterung of plates No. 1, giving rough a and δ and accurate estimates of photographic magn.	400 000
15. Measurement of plates No. 3. 1—4 images of 30 stars on each of 78 plates	5 000
16. Measurement of plates No. 2. 416 plates with 60 images to be measured on each	24 960
17. Measurement of plates No. 4. 226 plates with 90 images to be measured on each	20 340
Proper motions.	
18. Complete measurement of plates No. 5. A total of about 20 000 stars to be measured in two coordinates on a duplicate set of 233 plates	383 000[1]
Parallax.	
19. 8 images of a total of about 20 000 stars to be measured on a threefold set of plates.	576 000[1]
Class of Spectrum.	
20. 504 plates to be surveyed	?

---

[1]) These numbers suppose 20 percent of the plates measured in two positions differing 180° in orientation.

Radial velocities.

21. Measurement of standards . . . . ?
22. Measurement of 244 objective prism plates . . . . ?

If the necessary funds can be secured the Laboratory of Groningen is willing to undertake:

No. 13 for the Northern sky;

and further

Nos. 14—19. The provision must be made of course that the photographs satisfy the conditions discussed in what precedes.

Meanwhile, as there is as yet no certainty that the necessary supplementary funds can be obtained, I cannot *for the present* undertake more than about half this work. *At all events* it would be highly gratifying, with a view to a more speedy termination of the work, that other observatories, provided with good measuring apparatus, would also take a part in the work of measurement and reduction. I am happy to say that one offer of such cooperation was already received, as will appear from the letter of Prof. Comstock annexed.

XVIII. Results to be obtained.

The upshot of the whole work must be:

1. Catalogue of about 200 000 stars giving:

α_{1900}, δ_{1900} exact to about 0′1;
Photographic magnitude;
Visual magnitude.

Down to the faintest stars included, the catalogue must be *complete* within areas of accurately known extent.

2. Catalogue of about 20 000 stars giving:

$\left.\begin{array}{l}\alpha_{1900},\ \delta_{1900};\\ \text{photog. \& vis. mag.};\end{array}\right\}$ borrowed from preceding catalogue.

p. m. in α and δ with individual p. e. of about $\pm$ 0″008;
parallax „ „ „ „ „ $\pm$ 0″013.

Further I will assume that, for the class of spectrum we will get down to the stars of magnitude 9.5 (Harvard scale) and for the radial velocities to the stars of magnitude 9.0, half of which will show well measurable lines. With a field of 4° × 4° (or rather of radius 2°15′) we would then get:

3. Catalogue of about 20 000 star spectra.
4. Catalogue of about 5800 radial velocities.
5. Brightness of sky in its different parts.

XIX. Concluding Remarks.

In conclusion I wish to present a few remarks which did not find a fit place in what precedes:

1st. In general the photographs for the present plan, to repay the labour to be spent on them, must be of high standard. Careful guiding will be of vital importance.

Several of the big telescopes are not provided with guiding telescopes. The experience, however, gained for instance at the Yerkes Observatory (Astrophys. Journ. 20 p. 123 (1904)), shows that the simple device for guiding the plate proposed by Common (Monthl. Not. 49 p. 297) „insures a „guiding which is far superior to anything that could be done" (for very large telescopes) „by moving the entire telescope."

2. According to what has been said, only perhaps a fifth, or a sixth part of all the stars within our areas of $75' \times 75'$ (or within a circle of $41'$ radius) will be measured for parallax and proper motion. There thus remains on the plates an enormous mass of data, not used, on which we may draw in any case in which subsequent discussion leaves any doubt about the reality of some particularity or other in the results.

I think this an advantage that can hardly be overestimated.

3. Another extremely valuable advantage of this great liberty in the choice of the objects on the plate is, that we may arrange our choice in such a way that the reduction can be afterwards made with the greatest convenience and accuracy.

4. The plates to be taken for the present plan will enable us, with relatively very little pains, to realise a catalogue of faint *time* stars, distributed regularly over the whole of the sky.

For, as our plan will give very accurate p. m. for a great number of stars, it will be only necessary for the purpose either:

1st. that the position of a couple of the standard stars (No. 12 of Recapitulation) of each area be determined in the meridian with exceptional precision, or, better perhaps:

2nd. that a very accurate position of these stars be determined by measurement on the plates themselves.

Now that we know that personal error depending on magnitude is one of the principal sources of systematic error in the right ascensions, such a set must be considered as extremely valuable.

5. In order to get at the conditions of the problem of deriving the radial velocities of the stars from plates obtained with the objective prism, the somewhat complicated formulae for the course of the rays of light through the prism must be developed:

At my request Dr. de Sitter has begun to do so. — What follows is borrowed from his *provisional* results.

Method I (Orbinsky).

In the spectrum of a star A near the middle of the plate, take that line as origin of coordinates for which the prism gives minimum of deviation, and let n_0 be the index of refraction corresponding to that line.

Let the axis of the Y be parallel and the axis of the x be at right angles to the axis of the prism. Further, let x_1, x_2 be the x of two lines widely apart in the spectrum of any one star; X, Y the coordinates in fraction of the focal lenght, which the corresponding star would have on a photograph obtained without any prism, on which the star A is in the centre and the axes directed as the x and y axes.

V = the radial velocity of the star relative to that of the central star; $\beta = \frac{1}{2}$ refracting angle of prism.

Then we will have,

$$x_1 - x_2 = (K - MV)[1 - PX + QX^2 + RY^2].$$

There will be no terms of the order of X^3 and Y^3 so that this formula is accurate to terms of the 3d order inclusive. It is assumed that the line corresponding to the index of refraction n_0 lies with some rough approximation midway between the two lines measured.

In this formula the constants

P, Q, R are functions of β and n_0.

M is a function (given by Orbinsky) of the wavelength of the two lines and of the irrationality of the spectrum.

K is evidently the distance of the two lines considered, for the central star.

If therefore the central star is a standard star, K will become known; M will be known for each pair of lines measured. P, Q, R could be determined for once and all, either by accurate determination of β and n_0 or rather by a few exposures of some region rich in standard stars (Pleiades or Hyades?) if we might assume that n_0 and β are absolute constants. This assumption is certainly allowed for the very small terms QX^2 ad RY^2. For the term PX it seems better not to rely on this constancy but to determine P by the help of a second standard star.

The method would thus require two standards, widely apart in the direction of the x's.

Method II (Pickering).

$$HV = x' - x - K_0 - AX - BY - CX^2 - DY^2.$$

The coordinates are defined as in the preceding method. $x' - x$ is the distance, in the direction of the x's, of one and the same line in the

two spectra belonging to the same star. V is the radial velocity relative to that of a standard star at the origin of the coordinates.

K_0 is evidently the distance $x' - x$ for that same star.

A. In the application of the method in which the prism is turned through 180°,

(1) . . . . $B = bf(n_0 \beta)$,

b being (see figure p. 49) the small distance in the direction of the Y's of the couples of corresponding spectra.

The quantities b being kept very small, we may certainly assume in (1) $f(n_0 \beta)$ to be an absolute constant, to be determined once for all.

The same may be said of the constants C and D, which are functions of n_0, β and n.

The last quantity represents the index of refraction corresponding to the lines under measurement. The values of C and D will therefore have to be determined, once for all, for several values of n.

The constant A is function of the quantities n_0, β, but also of such quantities as: changes caused by the turning of the prism in the coordinates of the line of minimum deviation in the spectrum of the central star; in the line of minimum deviation itself, etc.

It seems hardly possible to determine this constant in a reliable way otherwise than by the introduction of a second standard star having a considerable X.

The use of *two* standard stars for each plate seems thus to be unavoidable.

B. In the application of the method, in which the prism is not touched, but the telescope used in its two positions and the plate turned through 180°, it seems not so certain that the determination of the constant B from the distances b in the direction of the Y of the corresponding spectra (which distances will now probably vary sensibly with the position on the plate) will be quite satisfactory. A standard star, differing widely from the two former ones in the Y coordinate, would of course obviate the difficulty.

On the other hand everything depending on the adjustment of the prism disappears from the constant A. Probably it will be possible, by careful adjustment and guiding, to keep both constants A and B within very narrow limits, which cannot be said of the constant A in the first method.

Meanwhile it may be very hard, in many cases, to find as many as three standards in good positions.

Turning the prism through 180° might seem therefore to be the preferable method.

Still the other way need not fail, even in the absence of a good third standard.

For, as the *two* standard stars (perhaps even *one*) certainly enable us to determine radial velocities for all the stars on the plate *by the Orbinsky effect*, we may derive the value of B from a number of well chosen stars for wich the radial velocity is first determined by Orbinsky's method.

Of course this possibility of using the same plates for a determination of radial velocity by both methods is extremely precious. The results obtained in such different ways will furnish an invaluable mutual check.

6. If a sufficient number of nebulae is obtained on the plates, it will certainly be advisable to measure at least their p. m. A few words about this matter were said before (p. 23).

The all important question, however, the solution of which might eventually be found by these measures, was not there mentioned. I consider this question to be the question of the absorption of light in interstellar space. — In Astr. Journ. No. 566 (1904) I tried to show, both the paramount importance of the investigation of this element, and at the same time its exceeding difficulty. — After that article was written the idea struck me that the nebulae might probably offer the best means of attacking the problem. For it is evident that in the case that there is no absorption of light, the surface brillancy of the nebulae will not vary with the distance, whereas if there is absorption it will do so.

If, therefore, we assume that the intrinsic brillancy of the nebulae does not vary *systematically* with the distance, if further we succeed in separating them in classes of different distance from the solar system, then the differce in the average apparent surface brightness between these classes will furnish the data for a determination of the quantity of the absorption.

I already tried to discover traces of absorption in this way, by comparing the brightness of the very small nebulae, in Dreyers General Catalogue, to that of the more extended ones.

On an average the former must be at the greater distance, so that, if there is absorption, we must expect to find their apparent surface brillancy to be inferior to that of the larger ones.

The trial proved with evidence, however, that the estimates of brightness in our catalogues of nebulae (at least of the smaller nebulae) is far more nearly an expression of the total light quantity than of the surface brillancy. — We may expect, however, that an investigation of the images of the nebulae on photographic plates, expressely made with a view to this question, might lead to intensely interesting results.

More satisfactory however, and leading to *quantitative* results, must be the result of such an investigation if, instead of judging the distance by the linear dimensions, we can derive it from the parallactic motion.

That the task is not at all a hopeless one appears sufficiently from a note in the Proceed. of the Amst. Ac. of Sciences, Meeting ~~22 March~~ 4 Feb. 1906.

7. With a view to the importance for the present plan of the question of keeping undeveloped plates for a long time I took the liberty of adressing myself to Messrs Lumière et fils.

These gentlemen very obligingly sent me the following answer:

„In order to keep the exposed plates, you must place them in the „boxes, the one against the other, film against film, without interposing „any paper or other substance between them. The plates must be first „cleaned carefully with a fine brush, in order to remove any particle of „dust which might leave an impression on the sensitive layer. The plates, „after having been packed in batches of 6, must be wrapped up in the „red paper which we use and must then be pressed strongly against each „other in order to prevent rubbing. The boxes must then be kept in a „dry and cool place.

„In these conditions the plates must keep a long time with all their „original properties."

Another authority on these matters recommends to keep the plates in boxed lined with glass on the inside.

Considering the evidence already obtained I think that (as for the present plan the pressing together of the plates might be dangerous) we may be pretty certain of keeping the plates in perfect order, if they are kept in metal boxes lined with glass or china and provided with grooves of the same material.

The utmost care has to be given of course to protect the plates from any light or dust. Prof. Donner always solders up his boxes.

Finally I will here transcribe a few lines from *Der Photograph* 1906 No. 10 p. 41 which proves that, if kept with due care, sensitive plates will keep their qualities even for a period of 18 years.

Wohl die längste Dauer der Haltbarkeit von Trockenplatten dürfte die Feststellung von Mr. R. Gibson sein, der seit dem Jahre 1887 Trockenplatten einer englischen Fabrik aufbewahrte. Als er diese aus der ersten Zeit der Trockenplattenfabrikation stammenden Erzeugnisse zufällig zur Hand bekam, trieb ihn die Neugierde, es zu versuchen, ob diese noch brauchbar seien. Er exponierte nun eine Platte, und diese gab zu seinem Erstaunen ein ebenso kräftiges Negativ, wie eine frische Platte. Als Beispiel für das gute Resultat sandte er dem British Journ. of Phot. einen Abdruck, dass die gute Qualität des Bildes bestätigt. Dieses Beispiel, dass Platten nach 18 jähr. Lagern noch trefflich verwendbar sind, dürfte wohl alle anderen bisher vorliegenden Meldungen von langer Haltbarkeit photographischer Trockenplatten übertreffen.

GRONINGEN, March 1906.

CENTRES FOR SYSTEMATIC PLAN.

No.	Magnitude H 24	Magnitude BD	Magnitude AGC	α 1875 (h m s)	δ 1875 (° ′)	Gal. lat. (°)
1*	7.09	7.0	—	7 29 —	+88 59.7	+28
2*	8.57	8.5	8.6	0 3 31	+75 7.9	+13
3	8.67	8.5	8.5	4 4 33	74 54.2	+17
4	8.47	8.5	8.3	8 1 17	74 51.9	+32
5	8.57	8.7	8.7	12 27 8	75 10.9	+42
6	8.82	8.5	8.4	16 14 17	74 58.1	+36
7*	8.87	8.8	8.4	20 24 45	75 2.4	+20
8*	8.21	8.3	7.8	0 58 6	+60 6.1	− 3
9*	7.26	7.0	7.5	3 1 34	60 9.5	+ 2
10	8.66	8.7	8.7	5 5 31	60 7.4	+13
11	7.31	7.3	7.4	7 4 34	59 59.1	+26
12*	8.21	8.4	8.4	9 0 47	59 45.5	+41
13*	7.31	7.3	7.1	11 1 26	59 53.4	+53
14*	—	8.7	8.8	13 21 24	59 32.9	+57
15*	8.56	8.1	8.1	15 16 1	59 58.5	+48
16*	—	8.2	8.1	17 28 21	59 47.0	+33
17	7.76	7.5	7.4	19 22 19	60 6.1	+19
18	8.56	8.7	8.4	21 23 47	60 0.1	+ 6
19*	—	8.8	8.9	23 21 54	59 53.1	− 1
20*	8.67	8.6	8.6	0 38 57	+45 12.7	−18
21	8.82	9.0	8.0	1 34 21	44 56.6	−17
22*	8.12	8.0	8.1	2 36 34	45 4.1	−13
23	7.67	7.8	8.0	3 36 51	44 57.5	− 7
24*	—	8.6	8.8	4 36 50	44 51.6	0
25*	—	8.5	8.7	5 35 34	44 46.8	+ 8
26	9.37	8.8	8.9	6 34 30	44 50.4	+18
27	8.97	8.8	8.8	7 36 25	44 52.4	+28
28*	9.20	9.0	8.8	8 38 8	45 1.9	+39
29	8.37	8.0	8.2	9 36 55	44 59.4	+49
30*	—	8.8	8.7	10 35 52	45 16.9	+60
31	8.62	8.5	8.6	11 35 34	44 50.2	+68
32	8.92	8.7	8.7	12 49 23	44 58.3	+72
33*	—	8.3	8.7	13 49 6	45 20.2	+68
34	8.02	7.3	7.8	14 46 53	45 7.4	+59
35*	9.12	8.5	8.5	15 48 40	44 53.7	+49
36	—	8.4	8.3	16 45 14	45 25.2	+39
37*	9.07	8.7	9.0	17 47 47	45 0.9	+28
38	7.62	8.3	8.3	18 45 28	45 10.8	+18

No.	Magnitude H 24	Magnitude BD	Magnitude AGC	α 1875 (h m s)	δ 1875 (° ′)	Gal. lat. (°)
39*	—	8.2	8.1	19 46 4	+44 50.1	+ 9
40*	—	8.7	8.8	20 45 41	44 52.8	0
41	—	8.7	8.7	21 48 37	44 51.9	− 8
42*	8.02	8.1	8.1	22 47 38	45 1.7	−13
43*	—	8.5	8.5	23 48 39	44 41.9	−16
44	8.66	8.6	8.6	0 22 47	+30 2.6	−32
45*	8.41	8.3	8.0	1 24 45	29 58.8	−32
46	8.51	8.2	8.2	2 27 58	30 7.9	−27
47*	7.06	6.8	6.8	3 21 7	29 56.4	−21
48	6.31	6.0	6.0	4 20 59	30 4.9	−12
49*	—	8.7	8.7	5 22 4	29 43.5	− 2
50*	8.06	8.3	8.4	6 22 47	29 54.6	+10
51*	—	9.1	{9.1 9.1	7 22 46	30 5.2	+22
52	—	8.1	8.1	8 24 11	30 7.8	+34
53*	—	8.6	8.5	9 23 5	30 7.2	+47
54	—	8.8	8.6	10 22 43	30 9.0	+60
55*	—	8.6	8.6	11 28 28	30 7.0	+74
56	8.46	8.0	8.0	11 56 54	29 49.0	+80
57*	8.61	8.1	8.1	13 2 39	30 3.2	+85
58	—	8.0	8.0	13 58 51	29 44.6	+73
59*	8.96	8.5	8.4	15 0 52	29 55.0	+59
60	8.61	8.3	8.3	15 55 10	29 59.3	+48
61*	8.06	8.4	8.4	16 57 52	29 59.9	+34
62	6.86	7.7	7.7	17 54 16	30 3.5	+23
63*	8.96	8.9	9.0	18 59 12	29 59.0	+10
64*	—	8.1	8.1	19 56 32	29 52.4	− 1
65	8.91	8.7	8.7	20 58 13	29 59.7	−12
66*	9.16	8.8	8.9	21 57 14	30 0.9	−20
67*	8.26	8.0	8.0	22 59 45	30 2.9	−28
68*	8.14	8.2	8.2	0 10 8	+15 8.5	−47
69	7.53	7.2	7.2	1 14 1	15 2.4	−47
70*	7.94	7.6	8.0	2 14 57	14 56.8	−42
71	8.74	9.0	8.6	3 10 14	14 56.7	−34
72*	6.49	6.3	6.0	4 8 41	15 5.2	−24
73*	8.24	7.9	8.2	5 13 28	14 55.8	−12
74*	9.14	8.8	8.8	6 13 49	15 6.4	+ 1
75*	8.59	9.0	8.4	7 13 32	15 7.8	+14
76*	9.14	9.0	8.7	8 14 9	15 6.1	+27

* The numbers of the areas having precedence are marked with an asterisk.

No.	Magnitude H 24	Magnitude BD	Magnitude AGC	α 1875	δ 1875	Gal. lat.
				h m s	° ′	°
77	—	8.3	8.4	9 9 15	+14 39.2	+39
78*	—	8.5	8.2	10 12 33	15 17.7	+54
79	8.49	8.1	8.4	11 16 23	15 0.5	+67
80*	—	8.2	8.0	12 11 16	15 8.3	+76
81	9.14	8.9	8.8	13 10 48	14 49.9	+76
82*	—	8.2	8.4	14 13 28	15 30.7	+66
83	8.44	8.3	8.6	15 7 56	14 55.0	+54
84*	—	8.3	8.7	16 11 2	15 6.1	+40
85	8.29	8.1	8.6	17 9 45	15 3.3	+27
86*	9.19	9.0	{8.8 / 9.1	18 10 37	15 1.8	+15
87*	8.04	8.2	8.6	19 10 7	15 0.3	+ 1
88*	8.54	8.5	8.7	20 9 48	15 3.4	−12
89*	8.09	8.1	8.4	21 7 0	15 4.5	−23
90	8.94	8.7	8.8	22 11 34	15 5.9	−34
91	—	8.2	8.4	23 11 54	14 55.2	−43
92	8.18	7.9	8.6	0 48 37	+ 0 6.8	−62
93*	9.33	8.9	9.0	1 48 26	+ 0 10.2	−58
94	8.58	8.6	8.7	2 49 32	+ 0 0.5	−49
95*	8.73	8.3	8.6	3 47 51	− 0 4.9	−38
96*	8.88	8.4	8.9	4 46 26	− 0 5.5	−25
97	6.78	8.1	8.0	5 51 1	+ 0 0.6	−11
98*	8.28	8.7	8.7	6 45 47	− 0 8.9	+ 1
99*	8.53	8.7	8.8	7 48 24	− 0 10.1	+15
100	8.43	8.2	8.4	8 47 36	− 0 8.4	+28
101*	—	8.3	8.4	9 50 15	+ 0 7.9	+41
102	9.01	8.7	8.8	10 49 1	− 0 8.8	+52
103*	—	8.3	8.4	11 48 37	+ 0 8.4	+60
104*	—	8.8	9.0	12 36 42	+ 0 8.8	+62
105	—	8.8	9.0	13 31 23	+ 0 0.7	+60
106*	—	8.7	8.9	14 35 14	+ 0 6.2	+51
107	7.63	8.0	8.2	15 32 38	+ 0 6.0	+41
108*	8.13	8.6	8.8	16 30 57	− 0 9.4	+28
109	8.53	8.2	8.5	17 38 26	− 0 4.7	+14
110*	9.08	8.7	8.5	18 35 54	+ 0 2.2	+ 1
111*	7.38	7.0	7.2	19 31 58	+ 0 3.8	−11
112	9.03	8.6	8.7	20 36 0	+ 0 0.0	−25
113*	8.28	7.8[1]	8.7pr	21 36 3	− 0 7.3	−38
114	8.48	8.4	8.6	22 36 0	+ 0 7.1	−49
115*	8.68	8.9	8.8	23 36 51	+ 0 12.7	−58
116	9.25	8.5	A 8.5	0 11 44	−15 0.5	−76
117	9.11	9.0	K 9.0	1 10 51	14 50.9	−76
118*	8.41	7.3	A 8.0	2 14 9	14 58.5	−65
119	8.01	7.5	B 8.0	3 8 58	14 54.5	−54
120*	—	8.7	A 8.5	4 11 1	15 10.2	−40
121*	8.16	8.3	Sj 8.5	5 13 45	14 54.0	−26

No.	Magnitude H 24	Magnitude BD	Magnitude AGC	α 1875	δ 1875	Gal. lat.
				h m s	° ′	°
122*	7.91	8.5	A 9.0	6 11 11	−15 5.0	−13
123*	8.11	8.5	B 8.5	7 14 4	15 1.9	0
124*	9.06	8.8	—	8 12 30	15 5.8	+12
125	8.06	8.2	—	9 11 9	14 59.0	+24
126*	7.01	7.0	G 8.5	10 14 47	14 51.7	+35
127	8.96	8.5	—	11 11 46	15 8.5	+42
128	8.66	8.2	B 8.0	12 12 31	15 5.5	+47
129	—	8.6	K 9.0	13 12 39	14 54.7	+47
130*[2]	8.56	8.3	A 9.0	14 6 24	15 2.3	+43
131	8.21	8.3	A 8.7	15 10 3	15 6.9	+34
132*	7.41	7.8	A 7.7	16 10 16	15 0.9	+24
133*	8.61	8.5	B 8.0	17 11 15	15 3.7	+12
134*	7.86	8.3	A 9.0	18 8 54	14 58.3	0
135*	8.91	8.8	A 8.0	19 11 33	15 0.6	−13
136	—	8.3	A 8.5	20 8 21	15 9.6	−26
137*	8.41	8.5	B 8.5	21 8 35	14 55.1	−39
138	8.05	8.0	A 8.5	22 9 17	15 3.4	−53
139*	9.16	8.5	A 9.0	23 12 37	14 55.9	−66
	H 34	CPD	Th			
140*	8.08	7.6	7.5	23 59 46	−29 50.8	−80
141*	—	8.6	8.3	1 1 36	29 57.5	−85
142	9.13	8.8	8.8	2 2 0	29 55.7	−72
143*	—	9.0	8.3	2 58 29	29 43.9	−60
144	8.49	9.0	8.0	4 0 52	30 1.1	−46
145	8.59	8.8	8.0	4 59 7	29 59.4	−34
146*	7.90	8.6	7.7	5 58 0	29 59.8	−22
147	9.05	8.6	8.8	7 1 37	30 5.1	− 9
148*	—	8.6	8.3	7 57 26	30 4.1	+ 1
149*	8.80	9.0	9.0	9 3 20	30 2.6	+12
150	9.35	8.7	8.7	9 59 20	29 56.5	+21
151*	—	8.4	7.3	10 58 34	30 10.2	+28
152	8.46	8.8	8.0	12 23 35	29 51.7	+32
153*	8.23	8.3	8.4	13 20 47	30 8.2	+32
154	8.88	8.6	8.2	14 21 1	29 55.9	+28
155*	—	8.6	8.7	15 24 40	29 46.1	+21
156	—	8.6	8.5	16 21 59	29 47.6	+12
157*	7.94	8.2	8.5	17 23 35	30 8.9	+ 1
158	8.42	8.9	8.4	18 21 5	29 59.5	− 9
159	8.28	8.0	8.2	19 21 31	29 57.7	−21
160*	8.13	7.8	8.1	20 23 3	30 6.3	−34
161	8.52	8.4	8.0	21 26 27	30 1.2	−47
162*	—	8.7	8.7	22 22 29	29 42.9	−60
163	—	8.8	8.0	23 21 6	30 12.5	−72
164*	8.02	8.6	8.0	0 37 29	−44 48.5	−72
165	7.95	7.7	7.9	1 33 26	45 13.9	−70

* The numbers of the areas having precedence are marked with an asterisk.

1) No. 113. Brighter n. pr. component of double star, seen as one in Bonn.

2/ No. 130. South prec. of two stars both 9 in A.

No.	Magnitude H 24	Magnitude CPD	Magnitude Th	α 1875 h m s	δ 1875 ° ′	Gal. lat. °
166*	8.95	9.0	8.9	2 35 10	−45 1.2	−61
167	9.18	9.0	9.0	3 37 2	44 59.5	−51
168*	8.50	8.4	8.6	4 38 8	45 9.2	−40
169*	8.68	8.8	8.2	5 38 1	44 59.3	−30
170	8.74	8.2	8.7	6 37 36	44 59.7	−20
171*	8.54	8.2	8.7	7 36 10	44 52.6	−10
172*	8.19	8.6	8.5	8 38 39	45 0.9	− 1
173	9.08	8.6	9.0	9 34 4	44 56.1	+ 6
174*	7.48	8.1	7.5	10 36 17	45 7.1	+12
175	8.03	7.8	8.0	11 35 51	44 57.5	+16
176	—	8.7	9.0	12 45 20	44 48.7	+18
177*	—	8.4	8.8	13 46 56	44 46.6	+16
178*	—	9.0	8.4	14 46 1	45 19.8	+12
179*	8.98	8.6	8.6	15 46 4	45 5.4	+ 6
180*	8.56	8.9	8.6	16 44 37	44 52.7	− 1
181*	8.17	8.2	7.9	17 46 33	44 54.2	−10
182*	—	9.3	8.8	18 48 10	44 59.6	−20
183	9.27	8.8	9.3	19 51 31	45 1.1	−31
184*	9.01	8.4	9.2	20 47 12	44 56.7	−41
185	8.66	9.0	9.0	21 49 4	44 54.3	−52
186*	8.12	7.6	8.1	22 42 34	45 5.0	−60
187	—	9.0	8.9	23 45 18	45 12.0	−69

No.	Magnitude H 24	Magnitude CPD	Magnitude —	α 1875 h m s	δ 1875 ° ′	Gal. lat. °
188*	8.48	9.0	Z 8.5	1 21 1	−59 59.1	−57
189*	—	8.2	Z 9.0	3 23 27	59 43.5	−47
190	—	8.3	—	5 22 43	60 4.0	−33
191	8.37	8.6	Z 8.5	7 22 24	60 15.0	−19
192*	8.52	8.8	Z 8.3	9 25 36	59 53.6	− 6
193*	8.73	8.1	Z 8.5	11 25 47	59 57.7	+ 1
194*	7.72	8.4	Z 8.0	12 55 10	60 4.3	+ 3
195*	8.34	8.7	Z 8.5	14 55 23	59 58.5	− 2
196[1]	7.90	9.6	G 8.5	17 0 16	59 55.4	−12
197	8.42	8.8	Z 8.0	18 59 13	59 44.4	−26
198	—	8.6	—	20 57 12	60 2.1	−40
199	—	8.2	Z 8.5	22 56 13	60 11.2	−53
200*	—	8.9	—	0 20 0	−75 5.9	−42
201	9.58	8.9	Z 9.0	4 29 7	74 58.7	−35
202	6.83	7.1	G 6.9	8 29 32	74 56.6	−20
203*	7.92	8.5	Z 7.5	11 58 33	74 59.5	−13
204	7.76	8.1	Z 8.4	15 56 53	74 57.5	−17
205*	—	8.6	Z 8.5	20 1 39	75 11.7	−32
206*	—	9.2	—	3 21 32	−89 48.4	−28

* The numbers of the areas having precedence are marked with an asterisk.

1) red or variable ?

CENTRES FOR SPECIAL PLAN.

No.	Magnitude H 24, 34 45.	B.D.	A.G.C.	Th.	C.P.D.	Various	α 1875	δ 1875	Galact. l	Galact. b	Density of Galact. light.	Map.	λ	Areas for which p.m. or π plates
—	......	......	......	......	......	......	......	......	0	+ 1	Rift		...	—
							h m s	° ′					°	
1	7.22	7.4	7.3				18 54 20	— 0 38	0	— 3	IV	Barnard	31	π
2	8.38	8.1	8.4				19 7 35	— 0 5	3	— 6	III	id.	33	p.m.
3		7.8	8.4				18 31 12	+ 7 26	4	+ 6	IV	Carte du Ciel Easton	25	π
4		8.6	8.8				18 55 17	+ 4 5	6	— 1	0—I	Barnard	28	π
5	7.93	7.7	8.3				19 35 11	+10 24	15	— 7	III—IV	id.	28	p.m.
—	......	......	......	......	......	......	......	......	17	+ 1	II	id.	...	—
6		7.9	7.8				19 18 31	+17 47	20	0	0	id.	19	π
7	7.26	7.5	7.5				19 32 32	+29 20	31	+ 3	V	id.	17	π
—	......	......	......	......	......	......	......	......	35	— 1	III—V	id.	...	—
8		8.1	7.8				19 48 31	+37 4	40	+ 4	V	id.	22	π
—	......	......	......	......	......	......	......	......	52	0	III—VI	Wolf Knowlegde 1891	...	—
9	7.42	7.5	7.8				21 2 44	+44 10	54	— 3	IV—V	„ Publ Königst Stuhl	36	p.m.
0		8.0	8.4				21 5 21	+45 53	56	— 2	V—VI	„ Knowledge 1891	37	π
1	8.02	7.7	8.2				20 59 18	+50 18	58	+ 1.5	0—I		38	π
2		8.5	8.6				21 55 49	+58 22	69	— 1.5	II—III	Barnard	47	p.m.
—	......	......	......	......	......	......	......	......	80	— 1	I—III		...	—
3					9.0	Z 8.5	0 44 56	—74 10	...	—43	....	Harvard 26	120	p.m.
4					8.5	Z 8.5	1 0 20	—72 55	...	—44	....	Harvard 26	122	p.m.
—	......	......	......	......	......	......	......	......	92	— 2.5	III		...	—
—	......	......	......	......	......	......	......	......	106	+ 2	I—II		...	—
5	7.96	7.4	8.1				3 29 47	+52 24	115	— 2	0	Easton	91	π
5		8.8	9.0				3 22 36	+43 47	118	—10	II		97	π
7		8.6	8.7				3 48 23	+36 47	126	—12	0—I	Barnard	106	p.m.
-	......	......	......	......	......	......	......	......	128	0	0—I	Easton	...	—
3		7.8	7.8				5 12 9	+40 45	134	— 3	IV	id.	108	π
-	......	......	......	......	......	......	......	......	144	— 2	II—III	id.	...	—
[illegible]		8.3	8.4				5 10 51	+27 57	145	— 5	0	id.	120	π
[illegible]	8.70	9.0				Bond 9.0	5 29 21	— 4 59	...	—18	....	Wolff	152	p.m.
[illegible]					8.9	Hersch 9	5 17 52	—69 43	...	—33	....	Herschel	139	p.m.
[illegible]					9.4	Hersch 9	5 37 4	—69 28	...	—31	....	and Harvard 26	139	p.m.
[illegible]		8.3	8.3				5 47 33	+30 41	147	+ 3	IV	Easton	119	π
[illegible]	......	......	......	......	......	......	......	......	163	+ 1	III		...	—
[illegible]		8.6	8.7				6 28 32	+10 56	169	+ 2.5	I—II	Barnard	139	p.m.

No.	Magnitude H 24, 34 45.	B.D.	A.G.C.	Th.	C.P.D.	Various	α 1875	δ 1875	Galact. l	b	Density of Galact. light.	Map.	λ	Areas for which p.m. or π plates are wanted.
							h m s	° ′					°	
25		8.7	8 4				6 34 28	+ 9 20	171	+ 2^5	o—I	Barnard	140	p.m.
—	......	......	......	......	......	......	......	......	180	+ 1	mean		...	—
26	6.40	7.2					6 19 34	— 3 49	180	— 6^5	poor-mean		152	p.m.
—	......	......	......	......	......	......	......	......	197	o	mean		...	—
—	......	......	......	......	......	......	......	......	215	+ 1	mean		...	—
27				7.3	7.6	Z 7.8	8 20 8	—40 20	226	— 1	mean-rich		152	p.m.
—[1]	......	......	......	......	......	......	......	......	232	— 1	rich		...	—
28				8.4	8.3	Z 8.5	9 4 41	—48 47	238	o	poor	Herschel	142	π
29					8.5	8.5	10 37 58	—59 15	255	o	rich !!	Gill	127	p.m.
30					8.0	Z 8	11 1 31	—61 11	258	— 0^5	rich poor	Harvard 26 Pl. V.	123	p.m.
—	......	......	......	......	......	......	......	......	261	+ 1	mean-rich		...	—
31					8 3	Z 9	12 48 26	60 14	271	+ 2^5	rich-poor	Herschel	111	p.m.
32	7.09	7.3	7.4				12 53 31	+28 44	...	+87		Wolf	68	p m. π
—[2]	......	......	......	......	......	......	......	......	272	+ 2^5	rich-poor		...	—
33	7.80				8.0	Z 7.8	14 24 30	—60 15	282	o	rich	Herschel	101	π
34					8.5	Z 8.5	14 44 40	—58 24	286	+ 0^5	poor	id.	98	π
—[3]	......	......	......	......	......	......	......	......	286	— 2	rich-poor		...	—
—	......	......	......	......	......	......	......	......	308	— 1	poor		...	—
35				8.1	8.0	Z 9	16 46 6	—28 45	321	+ 9	rich	Barnard	62	π
36				8 6	8.4	Z 8.8	16 26 18	—24 1	322	+15		id.	60	p.m.
37				8.8	8.6	Z 9	16 36 31	—23 46	323	+13^5		id.	58	p.m.
38	7.70			7.5	7.7	Z 7	17 2 20	—27 36	324	+ 7		id.	59	p.m.
39				8.2	9.1	Z 9	17 16 47	—26 48	325	+ 5		id.	58	p.m.
—	......	......	......	......	......	......	......	......	325	+ 1^5	mean	id.	...	—
40				8.8	8.8	Z 9.5	17 48 40	—29 2	328	— 2^5		id.	59	p.m.
41		8.8			8.7		17 11 10	—21 27	330	+ 8^5		id.	53	p.m.
42				8.9	7.8	Z 8.8	18 7 46	—28 7	331	— 6	rich	id.	58	π
43	6 45	6.7					18 10 9	—18 31	340	— 1^5	rich-poor	id.	49	p.m.
—	......	......	......	......	......	......	......	......	343	o	poor	id.	...	—
44		8.7				K 8	18 30 30	— 12 47	347	— 3^5		id.	44	p.m.
45		8.3	8.4				18 43 54	— 7 59	353	— 4	rich!!	id.	39	π
46	7.28	7.2					18 45 50	— 3 53	358	— 3		id.	35	p.m.

[1] Nearly coincident with de Sitter VII (Stars blue).
[2] border Coalsack.
[3] Near No. 34.

REMARKS.

Nº. 1. Rich area. Beyond the area both N und S much darker regions.

„ 2. There is a 8.6 South prec. Middle part of a curved dark fissure in M. W. extending over 3° or 4° in length and in parts over 10′ in breadth.

„ 3. There is a 9.2 North-prec.

„ 4. Rift. Very poor part. A rich part begins in the South following corner.

„ 5. Irregular large black hole (covering about half the area) in the midst of rich part of M. W. Position very favorable for reliable determination of relative p. m.

„ 6. Rift. There is a 9.2 3′ North.

„ 7. Very rich.

„ 8. Very rich part in Cygnus.

„ 9. Americ. Neb. (Publ. I Königstuhl p. 184). Centre of area in dark gap, of about half a degree in breadth between extremely rich regions.

„ 10. Extremely rich, near α Cygni.

„ 11. Northern coalsack in Cygnus.

„ 12. Irregular black hole (covering about a third of area) in the midst of pretty rich part of M. W.

„ 13 & 14. Nubec. Minor. The choice of the areas has been made, in consultation with Prof. Pickering, making use of his photograph in Harv. Ann. 26 Part II, Plate IV. By far the richest parts of the cloud are contained. It seems interesting to determine the p. m. both of the stars & the nebulae.

„ 15. Poor region in Perseus.

„ 16. Poor region around Nova Persei.

„ 17. Sudden change in stardensity. Southern part of area nebulous and very rich in stars. Where the nebula ceases there is a sudden fall in the stardensity.

„ 18. Rich region in Auriga.

„ 19. Poor region near β Tauri.

„ 20. Orion Neb. The area contains the trapezium in its Southern part. The centre is so chosen that great part of the nebula and further some regions of the very poorest as well as some of the very richest in stars are included. For this selection use was made of the counts and the map contained in Publ. I Königstuhl p. 180.

„ 21 & 22. In Nub. Major. *Vide* Herschel's Cape obs. p. 156 ff. and Harv. Ann. 26 Part II, Plate IV. The parts richest in stars & nebulae have been chosen. 30 Doradus is contained on area

No. 22, at a distance of 22′ (n.f.) from the centre. It seems interesting to determine the p. m. both of the stars and the nebulae.

N°. 23. Rich region in Auriga.

„ 24. Black part of M. W. surrounded on three sides by rich parts. Nebula in s. f. corner.

„ 25. Large black hole, somewhat along diagonal of area, surrounded by bright parts of M. W. In Northern part. Neb. near 15 Monocer.

„ 26 & 27. Area VI and VIII de Sitter. De Sitter finds stars of area 26 reddish, those of area 27, blue. See Publ. Astr. Lab. Groningen 12, p. 119. The determination of p. m. is of interest in connection with a possible difference in spectrum.

„ 28. „At this part... the continuity of the M. W. may be said to be „almost broken off" Herschels Cape obs. p. 384.

„ 29. Region of η Argus. There is a 8.7 n.f.

„ 30. Quick change in density near θ Argus.

„ 31. Sudden change in density on border Coalsack „Transition from „rich M. W. to almost complete darkness is here very sudden." Herschel Cape obs p. 384—85.

„ 32. Nest of nebulae near Pole M. W. On an area of 75′ × 75′ Wolf has photographed here about 400 nebulae (Publ. Königstuhl I 172).

„ 33. Near α Centauri.

„ 34. Between two bright branches, near α Centauri.

„ 36. Nebulous streamer in Scorpius 0°-6 in breadth, along diagonal of area (n.f. to s.p.) between dark gaps nearly devoid of any nebula and stars.

„ 37. Centre of area in dark gap in Scorpius, 0°7 in breadth, traversing the whole of the area from East to West and bordered North and South by rich regions. This gap is a prolongation of the Southern one of area N°. 36.

„ 38. Near θ Ophiuchi. Large black hole, in very rich part of M. W.; covers the greater part of the n.f. quarter of the area. There is another, smaller, hole near the Southern limit of the area.

„ 39. Part of large gap, in the mean about 0°6 in breadth, South of θ Ophiuchi (somewhat over 3° distant from N°. 38). The Southern and still more the Northern part of the area is very rich. — The centre is *not* chosen in the very poorest part but on a strip of stars, bordered East and West by very black space.

„ 40. Irregular black holes amidst very rich M. W. The region near following border is exceedingly rich.

„ 41. Near γ Sagittarii. Curved dark rift (South of η Ophiuchi), on an

average 0°4 in breadth. The s.p. quarter of the area and its n.f. part are very rich.

2. Extremely rich part of M. W. near γ Sagittarii, *Vide* Herschel Cape obs. 385.
3. Diagonal n.f. to s.p. extremely rich. In n.p. quarter of area two black holes; s.f. corner poor.
4. Dark rift, about 0°5 in breadth traverses area from North to South following. S.p. part of area very rich.
5. Extremely rich in Scutum.
6. Streamer of stars and nebula (about 0°5 in breadth) traverses area from East to West. Southern and still more Northern part of area very poor.

APPENDIX.

EXTRACTS FROM LETTERS.

(IN ORDER OF FIRST DATE)

Prof. Edw. C. Pickering,
Director,
Harv. Coll. Observ.

1904 Decemb. 16.

It does not seem to me possible to fulfil the two conditions that the two hundred regions should be equally distributed and also that they should include the most important portions of the sky. The Milky Way is not a great circle and its points of greatest and least density will necessarily occur at irregular intervals. There are certain centres of cosmical disturbance, like the Nebula in Orion, which should surely be included. Another near N.G.C. 6475, R.A. $17^h 45^m$, Dec.—35°, seems, on a map covering a large area, to have had an important influence on the structure of the Milky Way. At least one region should include portions of the sky where, as in Herschel's coalsack in Crux and the great nebulous region in Scorpius, stars are almost entirely wanting, apparently owing to large adjacent nebulae. I send you some photographs illustrating these points. How would it do to have two sets of regions, 1. Nearly uniformly distributed (the centres and corners of regions 30° square) for statistical purposes, 2. Special regions, generally maxima or minima, rather than average regions? As in making a contour map, we might take the height of points at the corners of squares a hundred metres on a side, but we should also take the top of each hill, the bottom of each lake, cols, ridges, ravines, and other distinctive points.

1905 June 14.

We are making some experiments with your method of determining magnitudes. The results seem to vary greatly with the distance of the screen from the plate.

1905 June 21.

I should like to undertake a large part of the determinations of photographic and visual magnitudes, the classification of the spectra, and photographic charts of the faint stars with the Bruce telescope . . . .

I satisfied myself that it (Orbinsky's method for measuring radial velocity) would not yield useful results. If tried, the prism should be set at the minimum of dispersion, not of deviation, that is, the front surface of the prism should be nearly perpendicular to the axis of the lens.

1905 Novemb. 8.

Your letter of October 25, and the copy of the proceedings of the Astronomical Society of the Atlantic duly reached me. I have read them with great care and interest. In a few days, I will send you a statement regarding them, any portion of which you may print if you wish. In general, I agree with your conclusions, and you may state that I am ready to undertake the part you have assigned to me*), subject perhaps to slight modifications. I hope to be able to prove, at least in theory, that if you can measure parallaxes with the accuracy which you expect, you ought to be able to determine the approach and recession of faint stars with reasonable accuracy, with an objective prism. I hope you will reconsider the plan of changing the centres of the regions to include a bright star, since this will introduce serious systematic errors. As your plan is now fairly before astronomers, it seems best to publish the work we have already done in this direction, and I shall try shortly to issue one of our circulars showing what progress we have made with the regions 30° square. We shall be ready to begin on our part of your work at once, or as soon as we can settle a few details.

1905 Decemb. 6.

Enclosed are the additional recommendations regarding the observations of the faint stars. I shall be glad to have you make any use of them you wish. Before long, I hope to send some photographs for you to measure. We shall have some taken specially for this purpose.

Durchmusterung Catalogues. A catalogue of the approximate positions and magnitudes of the stars in each region should be begun at once, to furnish the working list of the objects to be observed. We can furnish photographs with the Bruce telescope, faster than they can be measured. An exposure of two hours is recommended. This would show stars as

*) Durchmusterung plates; photographic and visual Standards; classification of spectra . . . . for both hemispheres. See letter 1905 June 21. J. C. K.

faint as Phoebe. A longer exposure would needlessly delay the work. We cannot take these photographs north of delination $+ 60°$, but could probably provide similar photographs with a smaller instrument by using longer exposures. The two polar regions are probably provided for by the Catalogues, Harvard Annals, Vol. 48, page 21, and Vol. 53, page 12, if they show sufficiently faint stars.

The number of stars per square degree on such Bruce plates varies from 30,000 in the denser portions of the Small Magellanic cloud, not including clusters, to 1800 in regions three or four degrees distant from it. It would be a pity to omit the fainter stars, or to photograph them if they are not to be used. It would seem best to reduce the regions from $75'$ square to $60'$ square, cataloguing the faintest in the central region $20'$ square. These regions should also be engraved on a scale of $20'' = 1$ mm.

Radial Motions of Faint Stars. It may be maintained that the radial motion of faint stars can be measured, with existing instruments, with an accuracy comparable with determinations of their parallaxes. Let us assume with Kapteyn that parallaxes can be found, with a probable error of $\pm\ 0''.04$, from photographs on a scale of $20'' = 1$ mm. The corresponding linear error is $\pm$ 0.002 mm. Also, that the expected parallax is about $0''.01$, but that by measuring many stars its mean value can be determined approximately.

As shown in Harvard Annals, Volume 26, Plate VII, spectra of stars of the ninth magnitude and brighter, over a region 10° square, can be photographed with an 8 inch telescope and objective prism, so as to give measurable spectra, about 6 mm. in length from $H\beta$ to $H\varepsilon$. The dispersion near the line $H\gamma$ would be such that 1 Ångström unit would equal 0.007 mm., and 1 km. approach or recession would therefore equal 0.0001 mm. The probable error of a measurement would therefore be about $\pm$ 20 km., or in general, nearly equal to its expected mean value, instead of four times as great as in the case of parallaxes. If the objective prism is used, as described in Harvard Circular 13, double the motion is measured for stars in all parts of the sky, but this may also be the case in measuring parallaxes of stars near the ecliptic. Taking successive groups of stars, the mean parallaxes become rapidly less as the stars grow fainter. There is no reason to suppose that this is the case with radial motions, a great advantage in measuring faint stars. In Circular 13 it was proposed to take two photographs, one being taken though the glass. It may be better to make two exposures on a single plate, reversing the second one by reflection from a plane mirror placed near it. *)

We have here considered a doublet of 8 inches aperture, and a prism

*) It is better to turn the prism 180°. Circular 110. E. C. P.

of about 13°. This angle could readily be increased to 30°, with a corresponding increase in accuracy. Care should be taken to place the prism at the minimum of dispersion, not the minimum of deviation. As a large region is covered, many spectra would be photographed, and those distant from the centre would determine with great accuracy the corrections to be applied to those near the centre. The advantages of a large reflector are very great, since there is no chromatic aberration. Instead of a large objective prism, a focal plane spectroscope, first tried here in 1893, and later successfully employed at the Lick Observatory, could be used to good advantage. Excellent spectra of much fainter stars could thus be obtained.

1906 March 13.

Your letter of January 10 is received. The plan which you outline seems to satisfy all the conditions as nearly as we can expect. I have accordingly written to Arequipa, asking to have some of the regions of the zone at 0° taken with the Bruce telescope. I think that, although the time of the instrument is already pretty well taken up, it will be possible, after a beginning has been made, to furnish you plates as rapidly as you will be able to measure them.

I have ordered the first ones made with one exposure of 120 minutes, and one of 120 seconds. It would be of the greatest value, if it were possible to derive the differences in magnitude of the different exposures, or even if we could be sure that the difference is the same for stars of different magnitudes. My experience has shown, however, that neither of these laws can be assumed.

It would seem better, therefore, if three exposures were to be given, to have them all of different lengths, e.g. 120^m, 5^m, and 1^m, but I think this is inadvisable, as it would add to the labor, and might cause confusion.

As soon as you have measured a few of these plates, you will see whether they are well suited to your purpose.

Prof. Edwin B. Frost,
Director Yerkes Obs^y^.

1905 March 9.

I fear that the prospects of obtaining reliable radial velocities from plates taken with the objective prism are very poor. I looked into the matter myself pretty carefully a dozen years ago (see *the Observatory*, vol. 18 p. 394, 1895); and then thought that there might be some chance. But the more I have worked on line-of-sight determinations, the less confident I feel that results of any value can be obtained with the objective

prism. I doubt if it would be possible to determine the sign for velocities of under 20 kilometers, unless the spectra are all very near the center of the plate. You see I fear that optical distortions away from the center of the field will more than mask the relative displacements sought. The fact that the dispersion changes with the angle and with the plane of incidence, complicates the matter very seriously.

1906 Feb. 1.

I shall certainly wish to assist you with the resources of this Observatory in your scheme in so far as it is feasible for us to do so. I can at present assign to this work one night per week of the time of the 40 inch telescope. I doubt if it will be possible to increase this amount, although it might turn out later that one and a half nights could be for a while denoted to the work. But it would not be right for me to omit to tell you of some of the great difficulties in the execution of even a part of the parallax work.

(1) It appears that with the use of a color-screen, an exposure of not less than one hour will be necessary to secure stars of the 14th magnitude. (This is determined from plates of the Pleiades where the magnitudes are well established.) If the color-screen is not used, the time of exposure can be cut down, perhaps, to 40 minutes.

(2) The difficulties of making the different exposures on the same plate will be very serious.

(*a*) With a visual refractor, whether or not a color-screen is used, yellow-sensitive plates must be used. The introduction of the dye into the film greatly diminishes the keeping qualities of the plate. Thus, while any sensitive emulsion for ordinary photographs might keep very well for two of three years or even longer, there is a great danger of deterioration of isochromatic plates within a period of six months. Occasionally they hold good for a year, but this is quite unusual, and cannot be counted on in advance.

(*b*) In view of the danger of the sky clouding up and cutting short the two or three exposures, numerous plates would probably have to be repeated, or measured up for a single date.

(*c*) I think it would be very difficult to get 60 stars on the average field taken with our apparatus; and it will not be feasible for us to increase the size of the field *). It would therefore be desirable for us to try only rich fields at first.

J. C. K.

---

*) $36' \times 45'$.

Prof. F. Küstner,
Director Bonn Obs[y].

1905 March 30.

I have carefully studied your scheme for starting the investigation of the stellar system, which surrounds us, by the cooperation of a few astronomers, *resp.* observatories. I wish to express plainly my complete concurrence with your plan in its leading lines.

In my opinion there can be no doubt but that a *complete* investigation of the whole of the sky must be considered out of the question.

The enormous richness of the stellar sky and the abundance of problems which it puts to the well equipped modern astronomer is such as to exclude this course at once. It would require an army of astronomers, conparable in number to the military armies.

The relatively small number of astronomers — (small even in comparison with the students of other sciences) — and of observatories imperatively demands a suitable limitation of the labour.

This limitation must bear least of all on the accuracy. On the contrary we must attain to the utmost precision obtainable which the present methods and instruments.

Neither ought it to bear on the choice of the elements to be determined: As such I consider:

Visual and photographic magnitude,

Number of stars for each determined magnitude,

Proper motion,

Parallax,

Spectral type *resp.* colour,

Radial velocity.

The knowledge of each and all of these elements, separately and in relation with each other, is equally necessary for further progress.

We therefore cannot limit the labour otherwise than by giving up any idea of entending our research over the whole of the sky. We must be content with investigating completely some determined areas, which however must be distributed uniformly over the whole of the firmament, giving only some preference to the Milky way, its most conspicuous phenomenon.

I also agree with you in the number and distribution of these areas. They seem to me to be necessary and sufficient for drawing safe inferences concerning the whole of the sphere.

It might perhaps seem that the number of 224 areas *) is rather

*) In the provisional plan only the 206 areas of the *systematic* plan, together with 18 additional MW plates were proposed. J. C. K.

small; that they lie too wide apart and leave too much room for accidental deviation.

If in reality this proves to be the case, little harm will have been done for — as you point out yourself — this drawback can always be redressed by interpolating a new series of areas. We will then even have the advantage of the experience gained by the execution of the first part of the work. For the history of science shows that in such investigations we must learn by experience, often dearly bought.

And just for this reason it is expedient not to extend the labour too far at once. — I therefore consider it to be quite right, not to exaggerate the number of areas. We thus have a better guarantee for an happy issue of the undertaking, which by a much greater number might be seriously endangered.

To what you say about the separate parts I have also to add very little which is essential — the less so because the working out of details is better left to the astronomer who will execute these parts.

Such important parts as: the determination of a uniform scale for photographic magnitudes, in the way proposed by you and also the determination of radial velocities by means of the objective prism need preliminary experiments. As to the second, Pickerings method, as modified by you, seems to me very promising. (I would prefer the turning of the prism through 180°, to a change of position of the instrument combined with a rotation of the plate). It applies in an ingenious way the principle of the latent photographic images, which so well stood the test of experience in the case of your method of determining parallaxes and proper motions.

Astronomers possessing sufficiently powerful refractors, provided with an objective prism, should certainly try this method as soon as possible.

For the determinination of the parallaxes and the proper motions you have already provided us with an entirely reliable method, thoroughly tested by experiment. — For the proper motions a telescope like the Bruce telescope of the Harvard Observatory would admirably suit the purpose. For a large field would allow a good choice of Standard Stars to be observed in the meridian. The focal length of $3^{m}.4$ — supposing the images to be satisfactory — would allow of a sufficiently accurate measurement. In case of need the precision can always be increased at will by repeating the photographs after the lapse of the necessary time.

On the other hand such a focal length seems insufficient for the parallaxes. The time does not here intervene and we have to deal with exceedingly small quantities. In fact I think that a focal length of 5 metres must here be considered as a lower limit. — The upper limit will be

determined by the condition that the diameter of the field ought not to fal materially below 1°. For in this case the number of stars, particularl that of the brighter stars would become too limited in many regions of th sky. Moreover the very long telescopes will only offer advantages unde very good atmospheric conditions. On the whole favorable climatic cir cumstances, particularly in the early morning hours, will be of the utmos importance for the parallax-plates. For the condition that the parallacti displacement must be as large as possible and that at the same time whenever possible, the plates ought to be taken in the meridian, restrict the time of observations, both as to the time of the year and the time c the day, within very narrow limits.

The climate at Bonn is little suitable for this sort of work, even we leave out of consideration our high latitude and consequent unequa distribution of day and night. The weather is far too unreliable and i the clear mornings, few in number, the air is usually very unsteady The consequence is that the images obtained in the morning are, as rule, far worse and more diffuse than those obtained in the evening This of course increases the probable errors and may even give rise t systematic errors.

For the photographs which I made for the determination of th parallax of Nova Persei at the Bonn refractor, these climatic disadvan tages were slightly counterbalanced by the position very near the zenith In regard to the precision of the measures I can now give you the follo wing particulars.

In all I obtained 8 plates with: 3 exposures for the first, 6 for th second, and 3 for the third maximum, *i. e.* with *twelve* images of every sta on each plate. For measurement I selected 60 stars (Nova inclusive) betwee magnitude 9 and 11, lying within a circle of 40′ radius round the Nov These 60 stars have been measured on all the 8 plates, whether th images were good or bad. Also the *three* images were always all utilise The measures were made, quite independently and at different times, th first time by myself, a second time by my assistant Dr. Zurhellen.

In what follows I computed the probable error of a parallax, separate for each plate, on the unfavorable supposition that all the parallaxes, calculated, are simply errors of observation; in other words on the assumptio that all the real parallaxes are *zero*. As a check on the computations th probable error has been derived, as in your Publication No. 10 p. 32 ar 33, both from the 1st and from the 2nd powers of the residuals, accordir to the formulae:

(1) . . . . $(\varrho) = 0.845 \dfrac{\Sigma(\pi)}{\sqrt{n(n-3)}}$

(2) . . . . $\varrho = 0.675 \sqrt{\dfrac{\Sigma\pi\pi}{n-3}}$.

The number of the unknown quantities was 3 because we may admit, on account of the small extent of the field used and the small Zenith-distance, that a linear formula must be quite sufficient for the adjustment.

We find the following values:

	(ρ)			ρ					ρ'	ρ'_1
Plate	K	Z	½(K+Z)	K	Z	½(K+Z)	H	H/4		
1	±0″0162	143	153	±0″0157	150	154	2.628	0.657	± 0″0101	±0″0175
2	224	213	218	226	223	224	2.471	.618	138	239
3	191	188	190	195	198	197	2.618	.655	129	223
4	124	132	128	127	126	126	2.864	.716	090	156
5	195	190	193	194	184	189	2.709	.677	128	222
6	152	152	152	152	156	154	2.852	.713	110	191
7	150	142	146	147	142	145	2.649	.662	096	166
8	234	233	233	229	257	243	2.832	.708	172	298
Means	±0″0179	174	177	±0″0178	179	179			± 0″0120	± 0″0209
							excluding Pl. 8		± 113	± 196

As will be seen the p. e. derived from the first power of the residuals is not different from that derived from the squares. Again the values agree for the two observers. On the contrary the values are appreciably unequal for the different plates, an inequality due to the difference in the character of the images caused by variation in the quality of the air.

Having regard to this circumstance it might have been allowable, nay perhaps imperative, to exclude *à priori* certain plates from being measured at all. Moreover by some unexplained cause plate No. 8, of another emulsion than the rest, is so densely fogged that it was nearly black and could be measured only by making use of an exceedingly strong illumination. If I have measured it nothwithstanding this, it was only for the purpose of ascertaining what could be got in such unfavorable circumstances.

The next column in the preceding summary contains the coefficient:

$$H = \frac{t_2}{t_1}(h_1 - h_2) - (h_3 - h_4), \text{ where } t_2 < t_1.$$

Then follows, as in your paper l. c., the reduction factor H/4; the prob.-error $\varrho' = H/4 . \varrho$ and lastly

$$\varrho'_1 = \varrho' \sqrt{3},$$

which is the prob. error of a parallax for one series of images (4 images on one plate) in the case of a parallax factor = 1.

In the mean we get from the 8 plates

$$\varrho'_1 = \pm\ 0''0209$$

or, if we reject plate N⁰. 8

$$\varrho'_1 = \pm\ 0''0196.$$

I will remark finally that the measurement has been made with the ordinary Repsold measuring machine, which was designed for the measurement of coordinates by means of a *reseau.* Consequently the microscope has a low power (16) and the value of a revolution of the micrometer is not less than half a millimeter (10 Rev. = 1 reseau-interval). We have thus been compelled to handle the micrometer very carefully and to read the drum by means of a magnifying glass. With a more suitable microscope and a finer pitched screw the measuring error might still be slightly diminished. At all events we may maintain that the prob. error of a parallax, derived from 4 images on a single plate, in the case of a parallax factor = 1, is

$$\pm\ 0''02$$

at most, for the refractor and the average (*i. e.* bad) condition of the air at Bonn. In localities with better climatic conditions it must be found still smaller.

Finally in regard to the meridian observations of the standard stars. I definitively declare, in the name of the observatory of Bonn, my readiness to have the observation of about 1500 stars for the Northern Hemisphere (including the equator) made at the Repsold Meridian circle, for the epoch 1915. Of course I have to make a certain reservation as a necessary consequence of so long a time in advance.

Prof. George C. Comstock,
Director Washburn Observatory.

1905 October 7.

Upon my return home I have read with great interest your plan for an investigation of the structure of the stellar system, in the execution of which I should be pleased to take some part.

I have no alterations to propose in the general outlines of the plan but modifications of detail will, of course, arise in its execution. It does not seem to me profitable to consider these in too great detail at the present time. The cooperation which, personally, I am able to offer in the execution of the scheme is limited by the instrumental outfit at my disposal, which is not adapted to photographic or photometric work. I shall, however, be pleased to undertake a part of the measuring of the photographs if you so desire.

1906 Jan. 27.

I have your letter of January 7 and am very pleased to express a renewed adherence to your plan for statistical survey of the heavens. I entirely agree with you that it seems inexpedient at this time to attempt to allot to me any definite part of the programme of measurement. I shall be pleased, however, to cooperate either in this way or another which will come to your knowledge a little later. There will appear in the next number of the Astrophysical Journal a paper by myself setting forth in outline a method of determining the radial velocities of faint stars, which I submitted to the last meeting of the Astronomical and Astrophysical Society of America. The method there seemed to find favor among the astronomers and physicists, and if it should successfully withstand the criticism that I invite for it, I shall hope to make application of it along the lines you have in mind.

R. I. A. Innes,
Director Obs. Johannesburg.

1905 Nov. 15.

I have not yet made any more photometric observations, chiefly because in the evening, the Milky way is ill-placed — but these will be made in due course*).

If we ever get the telescope it will have but two duties as far as I am concerned — one is the double star work, the other the prosecution of your scheme as far as it is possible with such an instrument.

In particular I would like to undertake the estimation of the visual magnitudes for the areas of the Southern Sky, including those on the equator.

Sir David Gill,
Director R. Observatory,
Cape of Good Hope.

1906 Febr. 26.

You ask me to write a few lines to the proposal which you have prepared for an attack on the great problem of the Structure of the Sidereal universe — and as to the share which the Cape could take in the scheme.

---

*) These observations are relative to the brightness of the sky, which Mr. Innes undertakes to make for the Southern sky. J. C. K.

You already know that I am deeply interested in the work and that I sympathize with the general idea on which your programme is based, viz: that the broad features of the problem can probably be solved by confining the research to the stars contained in limited but symmetrically distributed areas; indeed it was in this direction that I long ago suggested a limitation of your originally proposed "parallax Durchmusterung".

For the accomplishment of such a scheme the cooperation of different observatories on a well considered general plan, adopted after full discussion with the concurrence of all concerned, is essential in order to secure mutual interest as well as efficiency and economy in the work.

On all these grounds therefore I welcome your plans and the Cape Observatory will cooperate in every way in its power. For some years to come its cooperation must be limited to the following classes of work:

(1) The parallax plates for the Southern Hemisphere and probably the measurement of a portion of them.
(2) The proper motion plates for the Southern Hemisphere.
(3) Such meridian observations as the future discussion of the programme may demand.

The Victoria telescope cannot be employed for either (1) or (2) as it is at present engaged in very rigorous determination of the motions in the line of sight of stars brighter than 3.0 mag. The gift of the Telescope by Mr. McClean was made specially with a view to its devotion to Astrographic work — and we cannot conveniently interpolate other work with our present staff and with the work before us. Thus for the parallax and proper motion plates we can only employ the Astrographic Telescope.

In order to enable you to judge of the suitability or otherwise of our Astrographic Telescope for the work I am sending you 2 plates taken with the Astrographic Telescope, each containing 2 exposures of the area whose centre is $\delta - 49^{\circ}$, α $7^{h}42^{m}$.

There are three exposures on each plate, each of half an hour — one taken with the full aperture of 13 inches, one with a circular aperture of $9\frac{1}{2}$ inches diameter and one with a circular aperture of 6 inches diameter. For plate 8740 the diaphragms were mounted close in front of the object glass and for plate 8741 the diaphragms were mounted on the dew cap, 18 inches in front of the object glass.

To enable you to form a better judgment about the magnitudes of the stars I am sending you an ordinary "Catalogue plate" of the same area taken with the three exposures of 6^{m}, 3^{m} & 20^{s}.

It is very dangerous to predict what may or may not be possible in the future, but I confess that I do not look forward with much hope of

POSTSCRIPT.

Just as the printing of the present paper was finished I received two letters from which the following passages are extracted.

Alex. W. Roberts, D.Sc.,
Lovedale, Cape Colony.

1906 May 10.

. . . . this letter is written simply to assure you of my desire to enter into cooperation with you, in the work you have in purpose.

. . . . My cooperation depends, as you say, upon my obtaining a 6-inch glass . . . .

I could use it, not only for your purpose, but also to follow the fainter variables down to 13.0 mag. At present I am seriously handicapped in my work by being compelled to lose the most of them when they get near the eleventh mag.

Arthur A. Rambaut, D.Sc., F.R.S.
Director Radcliffe Obs^{y},
Oxford.

1906 May 11.

As you know well from the many letters which have passed between us and the conversations we have had on the subject, I take the deepest interest in your great scheme for collecting statistical data, for large numbers of faint stars, with regard to the principal elements to which our knowledge can extend.

The very comprehensiveness and variety of your scheme make it difficult to express an opinion on your proposals as a whole, but I am greatly impressed with the magnitude of your design and the importance of the results likely to accrue from it.

I hope sincerely, therefore, that you may be able to induce a large ırmber of observatories to cooperate with you in carrying it out.

. . . .

And now with regard to the proper motion plates. I have been oking into the programme you sent me and trying to form an estimate the amount of work that its accomplishment would entail. If I underand rightly you propose that we should give such an exposure as would ake sure of all the 14th magnitude stars in each of the selected areas. I find that on a plate of the Nova Persei region, exposed under rather ıfavourable circumstances for 15 minutes, we get down to about the 13th agnitude, or rather beyond it, on the Harvard scale. This seems to dicate that we should have to give about 40 minutes exposure so as to sure the 14th magnitude stars. On the plate examined there were images many stars below the 13th magnitude, but they were rather too faint give satisfactory measures.

With such long exposures as 40 minutes we cannot hope to make ry rapid progress with your list.

Then the question arises as to what interval of time would you propose leave between the first and last exposures. In order to get a really od determination of the proper motion, it seems to me that we ought to ow hardly less than 10 years to elapse. But you know what my views as to the danger of allowing plates to lie by for so long between posure and development. Even in a year and a half some of our rallax plates have shown defects due to this cause, though every care s been taken to keep them air tight and dry.

After ten years I should quite expect to find a considerable proportion the plates defective, and probably the sensitiveness much diminished.

While, therefore, I am prepared to redeem my promise and to make self responsible for some of the proper motions, I would not like to ıd myself to take *all* the plates that you require, and I shall be glad you can find somebody who will share this undertaking.

Unless we were to devote ourselves entirely to this work for many ırs to come, I could not with my small staff undertake more than half total number of plates. If you were content to allow us to develope plates as they are exposed and to depend for the proper motion on a nparison of the measures made in the ordinary way on two plates intervals of 10 years, or on a composite negative made from two ginals, I do not think I should mind embarking on the whole, but I derstand that you consider it essential to get the exposures at the ginning and end of the period upon the same plate. Under these circumnces, making allowance for the number of plates that will be spoiled by

keeping and by accidents of various kinds, I think the work will prov a much bigger one than appears at first sight on paper.

I am, however, willing to undertake to procure 246 plates with tw exposures each in the way you propose, that is to say, one exposure a the beginning and one at the end of an interval of about ten years. I somebody else can be found who will duplicate the work you will hav all the material you require and the result ought to be better than if al had been done by the same observers and with the same instrument.